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ON THE DENSITY OF *ALGOL* VARIABLES[1]

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I. INTRODUCTION

It is a well-known fact that for a mass or system of masses of incompressible homogeneous fluid of density ρ, the value of ω^2/ρ depends solely on the shape and configuration of the masses, and not on the dimensions of the system. If we accept that explanation of the *Algol* variables which attributes the variation of the light to alternate eclipses of the two components of the system, then the knowledge of the value of ω, the angular velocity, which we deduce from the observed period of the light, leads to a certain inference as to the value of the density ρ, subject to the assumption that the matter may be treated as homogeneous and incompressible. It has been remarked that in almost every case which has been examined, this density is found to be surprisingly small. Generally, the calculated density is so small that the original hypothesis of an incompressible fluid is seen to be quite untenable; it is obvious that if the densities obtained are accurate, the system must be supposed to be in the gaseous state. This consideration tends to cast doubt over the whole investigation; for a gaseous mass is certainly neither incompressible nor homogeneous

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[1] I have used the term "*Algol* variable" as a convenient designation for all variables showing regular variations of light, although the variables with which the paper is principally concerned are not of the strict *Algol* type.

and, as we shall see later, if the original assumption breaks down, the relation between ω and ρ does not hold, even approximately. Indeed, the calculated values of the mean density may be increased almost without limit.

Also, in some cases, it is found that the two components of the system must be supposed to be almost or quite in contact. We ought, therefore, to consider the enormous tidal forces which will be at work, so that the system of forces exerted by the masses on one another will comprise not only the direct gravitational attraction which may be supposed to act between their centers, but also forces arising from the tidal distortion, and also possibly repulsive forces produced by the pressures exerted through the neck of fluid which joins the two masses, or through the intersection of their atmospheres.

The effect of allowing for tidal forces would be to decrease the value of ρ; but if a neck is formed, the pressure transmitted by this neck would tend toward the increase of ρ. The object of the present paper is not so much to explain how to allow for these various corrections as to examine to what extent we are justified in drawing any deductions from observations on the variables.

II. THE CONFIGURATIONS OF EQUILIBRIUM OF ROTATING INCOMPRESSIBLE FLUID SYSTEMS

We may begin by recapitulating what is known as to the relation between ω and ρ for systems of incompressible fluid. With no rotation, the configuration of equilibrium is a sphere. If a spherical mass of liquid is set gently in rotation, it assumes a spheroidal shape, and if the angular momentum of the rotation is increased, the spheroidal form is maintained until the rotation is such that the angular velocity is given by

$$\omega^2/2\pi\rho=0.18712 \ . \qquad (1)$$

These figures are the spheroids of Maclaurin. After passing the momentum given by equation (1), the figure is no longer spheroidal, but ellipsoidal, and remains ellipsoidal until the momentum is such that

$$\omega^2/2\pi\rho=0.14200 \ , \qquad (2)$$

this momentum being greater than that given by (1), although the angular velocity is less. These series of ellipsoidal figures are Jacobi's ellipsoids.[1]

When the angular velocity exceeds the critical value corresponding to equation (2), the configuration is no longer ellipsoidal, but is initially pear-shaped. This series of pear-shaped figures was first discovered by Poincaré,[2] and have been discussed by Poincaré and Darwin.[3] It is found that the angular velocity for these figures is less than that given by equation (2), although the angular momentum increases continuously. As the angular momentum further increases, beyond that corresponding to these pear-shaped curves, nothing is known as to the configurations which the mass of fluid will assume, beyond what may be inferred from an investigation of the corresponding problem in two dimensions.[4] From this and from various considerations of a general nature, it seems almost certain that the mass of fluid will ultimately split into two parts. The equilibrium of two such masses of fluid, assuming them to be nearly spherical, but allowing for their mutual tidal influence, has been discussed by Darwin[5] and Roche.[6] It appears that it is impossible for an infinitesimally small mass to rotate in contact with a primary; this is only possible if the mass of the satellite is at least equal to (about) one-thirtieth of that of the primary. Darwin shows that when the masses are rotating in contact, we have approximately

$$\omega^2/2\pi\rho=0.13\ , \tag{3}$$

while in the case in which the masses are equal, the value is approximately,[7]

$$\omega^2/2\pi\rho=0.09\ . \tag{4}$$

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[1] G. H. Darwin, "On Jacobi's Figure of Equilibrium for a Rotating Mass of Fluid," *Proc. R. S.*, **41**, 39.

[2] "Sur l' équilibre d'une masse fluide," *Acta Math.*, **7**, 3 and 4, 1885.

[3] H. Poincaré, "Sur la stabilité de l'équilibre des figures pyriformes," *Phil. Trans.*, **198** A, 333. G. H. Darwin, "On the Pear-Shaped Figure of Equilibrium," *ibid.*, **198** A, 301, and "The Stability of the Pear-Shaped Figure," *ibid.*, **199** A, 253.

[4] J. H. Jeans, "On the Equilibrium of Rotating Liquid Cylinders," *Phil. Trans.*, **200** A, 67.

[5] "On Figures of Equilibrium of Rotating Masses of Liquid," *Phil. Trans.*, 1887, p. 379.

[6] *Montpellier Acad. Sci. Mémoires*, **1**, 243.

[7] I find this value by interpolation between Darwin's two cases in which the masses are almost touching and are just more than touching. *Phil. Trans.*, 1887, Plate 22.

All these investigations, it will be noticed, take full account of the tidal influences and of the pressure of the neck of liquid where the two bodies are merged into one. Assuming the matter of which our system is composed to be adequately represented by a homogeneous incompressible fluid, the process which we have been tracing is that of the birth of a double star. The process may be divided into two periods. The first is terminated when the point represented by equation (2) is reached. Through this period the star is spheroidal or ellipsoidal, and the emitted light would, from the symmetry of the star's figure, exhibit equal maxima and minima. After this point is passed, we come to a period in which the figure of the star possesses only two, instead of three, planes of symmetry. Here, if we suppose the surface of the star to be of uniform brightness, we should expect the star, except in very special cases, to exhibit two equal maxima and two unequal minima. Hence it will be during this second period that the star will first appear as a variable of the *Algol* type. Up to the moment of separation into two masses, the emitted light will constantly change throughout the whole period. After this separation, the amounts of light emitted will remain stationary at the maxima and minima for certain times, these times depending on the extent of the separation.

III. CALCULATION OF ρ

Let us now consider the former class of variables, representing a star throughout what we have called the second period, up to its separation into two parts. At the end of the period, ω lies between the values given by equations (3) and (4). Throughout the intermediate period it seems highly probable that ω decreases steadily. But the only definite and accurate knowledge in our possession is that contained in equations (2), (3), and (4). Fortunately, these equations give approximately the same relation between ω and ρ. As an average, we may take

$$\omega^2/2\pi\rho=0.12 \ . \qquad (5)$$

We have to admit that this equation may be subject to an error of about 25 per cent., but it represents the most accurate knowledge at our disposal. It enables us to calculate ρ to within about 25 per cent.

If we use any units other than astronomical, we must replace ρ by $\gamma\rho$, where γ is the gravitational constant, of which the value is 648×10^{-10} in C. G. S. units. We have also $P\omega=2\pi$, where P is the period, so that equation (5) becomes

$$\rho=\frac{2\pi}{0.12\times\gamma P^2}=\frac{8\times10^8}{P^2}\text{ (nearly)} . \tag{6}$$

As an example, we may take the case of the star *β Lyrae*, belonging to this class of variable, for which $P=12.91$ days,[1] or $=1.11\times10^6$ seconds. The value of ρ given by equation (6) is

$$\rho=0.00065 , \tag{7}$$

agreeing closely with the value found by Professor Myers.

As a second example, let us take *V Puppis*,[2] another variable of this type, for which $P=1^d\ 10^h\ 57^m\ 26^s=125{,}846$ seconds. The value of ρ given by equation (6) is

$$\rho=0.051$$

a value comparable with, although far from equal to, that given by Roberts ($\rho=0.02$).

THE BIRTH OF A NEBULOUS DOUBLE STAR

The results just given show that the densities found by supposing the star to be liquid are much too small to belong to a liquid, so that our calculation is rendered nugatory. Further instances of densities too small to belong to liquid stars will be found in lists of calculated densities of double stars given by A. Roberts and H. N. Russell.[3] We accordingly turn to the consideration of a nebulous double star.

Here the birth of a double star is a process very different from that just considered.[4] Mathematically, the main distinction is that there is no longer any uniform relation between the density and the angular velocity of rotation, and, so far as can be seen, the process of separation may take place with an extremely small angular velocity. For the agency which effects the separation will no longer be rotation alone; gravitation also will tend toward separation. Indeed, a

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[1] G. W. Myers, "The System of *β Lyrae*," *Astrophysical Journal*, **7**, 1, 1898.

[2] A. W. Roberts, "On the Orbits of the *Algol* Variables," *ibid.*, **13**, 177, 1901.

[3] *Astrophysical Journal*, **10**, pp. 314 and 317, 1899.

[4] J. H. Jeans, "The Stability of a Spherical Nebula," *Phil. Trans.*, **199** A, 1.

nebula can split into two parts under gravitation alone, the two nuclei being held apart by the pressure of the layer of gas which separates them, instead of by the so-called centrifugal force.

From numerical results obtained in the various papers of my own to which reference has already been made, I have been led to the conclusion that a gravitational instability of the kind described must be regarded as the primary agent at work in the actual evolution of the universe, Laplace's rotation playing only the secondary part of separating primary and satellite after the birth of the satellite.

Let us go back to the primitive nebula, out of which our universe may be supposed to have grown. We may, for convenience, regard this nebula as a gas of uniform density, at uniform temperature throughout, and extending to an indefinite distance in all directions.

A gas in this state is obviously in equilibrium, so long as we do not take account of the existence of its boundaries. The equilibrium may, however, be either stable or unstable, and a brief calculation shows that it is unstable.[1] The gas will tend to form into clusters about a number of nuclei.

To obtain the most probable disposition in space of these nuclei is a problem of statistical mechanics, to which I have not tried to obtain an exact solution. If, however, a solution had been obtained, we should be able to calculate, among other things, the most probable mean distance apart of adjacent nuclei. This mean distance can depend on only three quantities, namely:

1. The gravitation constant, γ.
2. The density of the original gas, ρ.
3. The pressure of the original gas, p.

The only way in which these quantities can be combined so as to form a length is through the expression

$$\sqrt{\frac{p}{\gamma\rho^2}}\ ;$$

or, if we put $p=k\rho$,

$$\sqrt{\frac{k}{\gamma\rho}}\,. \tag{8}$$

Hence, by an argument from physical dimensions which I have

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[1] J. H. Jeans, *Phil. Trans.*, **199** A, p. 49.

explained elsewhere,[1] it follows that the mean distance apart of the nuclei must be comparable with expression (8).

Denoting this quantity by a, the mass M of the gas which concentrates about each nucleus must be comparable with ρa^3, and therefore with

$$\sqrt{\frac{k^3}{\gamma^3\rho}}\,. \tag{9}$$

Hence M/a must be comparable with k/γ.

To examine to what extent this result agrees with the formation of the universe as we know it, let us make use of Lord Kelvin's estimate as to star density, that there are 10^9 stars within a parallactic distance of $0''.001$ of the Sun.[2] Assuming these stars to be arranged in regular cubical piling, this gives $0''.62$ as the mean parallax of adjacent stars in terms of the earth's orbit, so that a is roughly $10^{18.7}$ cm. The value of γ is 648×10^{-10}. It is difficult to know what value to assume for k, but as a rough estimate we may take $k=10^9$, which would be the exact value for a gas of molecular weight 30 at a temperature of 350° absolute, or for hydrogen at a temperature of 24° abs.

The value of ak/γ is found to be $10^{34.9}$, and this, on the gravitational hypothesis, ought to be comparable with the *average* mass of a solar system. The mass of our own system is $10^{33.3}$, which, considering the extreme vagueness of the data, must be regarded as in good agreement with the number $10^{34.9}$. The agreement is vastly improved on taking an average parallactic distance greater than $0''.62$; I do not know what evidence there is as to the number of dark stars between us and the nearest stars which are bright enough for us to see them. Statistics of binary stars suggest that only a small proportion of the stars in the universe are bright.

As the evolution of the universe progresses, the original nuclei doubtless separate further into distinct nuclei, so that in any case there is no ground for surprise in finding that the gravitational theory predicts masses greater than that of our present system. The confirmation of the gravitational hypothesis must not be expected to

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[1] "On the Vibrations and Stability of a Gravitating Planet," *Phil. Trans.*, **201** A, 158.

[2] "On Ether and Gravitational Matter through Infinite Space," *Phil. Mag.* [6], **2**, p. 161.

lie in exactness of fulfilment of its predictions, so much as in the fact of its predicting enormous masses of quadrillions of quintillions of tons as the masses of the systems; the theory which relies on pure rotation cannot even predict the order of magnitude of masses to be expected.

The supposition that gravitational forces preponderate, not only in the formation of the original nuclei, but also in the evolution of a solar system out of each nucleus, leads to the prediction of a relation between the masses and densities of planets possessing satellites, provided that the planet was in a liquid or molten state when the satellite was thrown off.[1] By the method of dimensions, it is easily shown that the mass of the primary must be comparable with

$$\sqrt{\frac{\lambda^3}{\gamma^3\rho^4}} ,$$

where ρ is the density and λ the elasticity of the material of the primary. Assuming λ to have a constant value for all the planets of a solar system, we find that the mass ought to vary as ρ^{-2}, so that the product of density and radius ought to have the same value for all the planets of the system. This law is approximately obeyed in the solar system.[2] Moreover, taking $\rho=1$, $\lambda=10^{12}$ as rough values,[3] we find that the mass of a primary possessing a satellite ought to be comparable with $10^{28.8}$ grams. The mass of the earth is $10^{27.8}$, that of *Jupiter* is $10^{30.3}$.

Let us compare the predictions of the gravitational hypothesis with a similar prediction which can be deduced from the rotational hypothesis.

Consider a mass of gas, ultimately to form a solar system, consisting of N molecules each of mass m, moving with a veloctiy of mean square C, and occupying a cube of edge a in the primitive homogeneous nebula. The moment of momentum about an axis x through its center is

$$\mu_x=\Sigma m(vz-wy) ,$$

so that the square of this moment of momentum is

$$\mu_x{}^2=\Sigma m^2(v^2z^2-2vwyz+w^2y^2) ,$$

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[1] "On the Vibrations and Stability of a Gravitating Planet," *Phil. Trans.*, **201** A, 157.

[2] "On the Vibrations and Stability," *loc. cit.*, p. 175.

[3] $\lambda=10^{12}$ is approximately the value for steel.

and the square of the total momentum is

$$\mu_x{}^2+\mu_y{}^2+\mu_z{}^2=\Sigma m^2[w^2(y^2+z^2)+\ .\ .\ .\ .\ -2vwyz\ .\ .\ .\ .]\ .$$

Assuming both the velocities and positions of the molecules to be distributed initially at random, we find on summing,

$$\begin{aligned}\mu_x{}^2+\mu_y{}^2+\mu_z{}^2&=\tfrac{2}{3}C^2\Sigma m^2(x^2+y^2+z^2)\\&=\tfrac{1}{6}C^2a^2Nm^2\ .\end{aligned}$$

The square of the moment of momentum remains constant throughout the contraction of the gas, being equal to $M^2\theta^4\omega^2$, where ω is the angular velocity, and θ the radius of gyration at any time. Thus

$$M^2\theta^4\omega^2=\tfrac{1}{6}C^2a^2Nm^2\ ;$$

or, since $Nm=M$,

$$M\theta^4\omega^2=\tfrac{1}{6}C^2a^2m\ .$$

Let θ and ω refer to the epoch at which a satellite is first formed, then as regards order of magnitude, we have on the rotational hypothesis, as already explained, $\omega^2=\gamma\rho=\gamma M/\theta^3$, so that

$$\gamma M^2\theta=\tfrac{1}{6}C^2a^2m\ .$$

Taking as rough values, $\gamma=65\times10^{-9}$, $a=10^{19.6}$, and noticing that $\tfrac{1}{3}C^2=k=10^9$, we obtain

$$\theta=10^{-34.5}\ .$$

Thus, on this hypothesis, the contraction of the nebula surrounding a nucleus would have to continue until the mass of, say, $10^{33.3}$ grams had contracted into a linear dimension of about $10^{-34.5}$cm, and therefore to a density of about 10^{137}, before rotation alone could effect the birth of a satellite.

Two remarks must be made on the subject of this calculation. If we could assume that the original material of the universe was in the solid state before the evolutionary processes began, then the calculation would be less unfavorable to the rotational hypothesis. We should have to suppose the gas of our calculations replaced by a swarm of meteorites in the solid state, each meteorite figuring as a molecule of a quasi-gas. This conception of cosmic evolution has been put forward by Professor Norman Lockyer,[1] and has been to some extent developed by Professor G. H. Darwin,[2] but, in view of

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[1] *Proc. R. S.*, **117**, 1887.

[2] "On the Mechanical Conditions of a Swarm of Meteorites," *Phil. Trans.*, 1889, p. 1

more recent knowledge and spectroscopic evidence, it can hardly be regarded as probable. The stellar systems which appear to be in the earliest stages of development mostly seem to be in a gaseous state.

Again, the amount of angular momentum predicted by the foregoing calculation is small compared with that possessed by our system. But as soon as we admit gravitational instability as a possibility, we must suppose variations of density to occur at an early stage of the evolutionary process, and the tidal influence of one nucleus on another will tend to increase the angular momentum of both. Indeed, by a well-known theorem of statistical mechanics, the tendency will be toward a continual increase of angular momentum until the energy of this momentum becomes equal to the average energy of proper motion of each nucleus, or, of course, until the condensation of the nuclei has proceeded to such an extent that their mutual tidal influence becomes negligible.

There is, therefore, I think, sufficient evidence, not only that gravitational instability may not be neglected, but that it is the principal agency to be considered in discussing questions of cosmic evolution. As regards the special question of the *Algol* variables, it becomes at once obvious that the smallness of the calculated density is merely a consequence of the neglect of the most important factors in the question. Knowing nothing but the period of rotation, we may not legitimately draw any inference as to the structure of the system.

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May 10, 1905.

THE FIGURE OF THE SUN

BY CHARLES LANE POOR

The following investigation of the figure of the Sun was suggested by the number of solar photographs taken by Lewis M. Rutherfurd in his private observatory and by him presented to the Observatory of Columbia University. In a series of investigations of the Rutherfurd star plates, Jacoby has shown that the plates have suffered no deterioration, and that they are capable of giving results comparable in accuracy with the best heliometer determinations. It was hoped, therefore, that the Rutherfurd photographs of the Sun would serve to determine with great precision the shape of that body.

After the investigation was well under way and some preliminary results were obtained, I decided to compare them with Auwers' reductions of the heliometer measures made in connection with transits of *Venus* in 1874 and 1882. This comparison led to some interesting results, which are given in the second section of this paper.

RUTHERFURD PLATES

The Observatory of Columbia University has in its possession a series of 139 solar photographs taken by Rutherfurd during the years 1860–74. This series of plates may be divided into two groups; one group of plates covering the years 1860 to 1866, and a second group taken during the years 1870–74. The plates of the first series were made with a small lens; those of the second group, with his 13-inch photographic objective, which was completed in 1868. The earlier plates were simple photographs of the Sun, without orientation marks or data of any kind. In 1870 he began to place orientation marks on the plates, but even after that date fully one-half of the plates lack this essential. In these four years Rutherfurd took one hundred plates, grouped as follows:

1870, February 16—October 14	61 plates
1871, April 17—August 19	14 "
1872, January 2—November 27	13 "
1874, April 5—December 9	12 "
Total	100 plates

Of the sixty-one plates taken in 1870, only four were available for measurement, the remaining plates not having sufficient data to orient them. These four plates were rejected in the preliminary investigation, but were afterwards measured and found to give satisfactory results.

Of the fourteen plates taken in 1871, eight were found to be measurable. A ninth plate was measured, but the measures were so discordant that it was rejected. The remaining five plates were either poorly developed, or did not have orientation marks upon them.

Of the thirteen plates taken in 1872, ten were found to be measurable. An eleventh plate was discarded after measurement, the separate measures being very discordant.

Of the fourteen plates taken in 1874, only one had on it the full data for orientation. This was a very poor plate, and was discarded after an attempt to measure it.

This left available for measurement a series of twenty-two plates, of which four were taken in 1870, eight in the spring and summer of 1871, and ten in the spring and summer of 1872. The measures were all made on the Repsold measuring-machine of Columbia University, and all measures were made in duplicate by Miss Harpham and Miss Davis, of the Observatory computing staff. On each plate twenty-eight points on the Sun's limb were measured—seven points at or near each pole, and seven points at or near each extremity of the equator. In each of these four groups the separate points were five degrees apart, each group thus covering an arc of 30° or 15° on each side of the pole or equator, respectively. The measurement of each point consisted in the determination of its polar co-ordinates, position angle and distance, as referred to the center of revolution of the plate in the machine. This center of revolution does not coincide with the center of the Sun's disk, but the plates can be quite accurately adjusted in the machine. In no case did the center of revolution differ from the true center of the disk by more than 0.05 mm, or $1'.2$.

The measures were corrected for differential refraction, the formulas as given by Chauvenet being used. From these corrected measures were then found the co-ordinates of the center of the Sun and the most probable value of the Sun's radius. The measured co-ordinates were then transferred from the center of revolution to the center of the Sun's disk as origin, and thus were found, for each plate, the values of twenty-eight radii of the Sun.

The mean of the fourteen polar radii, as thus found, for each plate, was taken as the value of the polar radius, and the mean of the fourteen equatorial radii as the value of the equatorial radius for that plate. The difference between these values of the polar and equatorial radii was then formed, in the sense polar minus equatorial, and the results for the various plates are exhibited in the following tables:

1870

Date	P.-E. (arc)		Wt.
Aug. 18........	+0″.49	±0″.28	2.4
Sept. 24........	−0.12	±0.31	2.0
Sept. 28........	+0.81	±0.25	3.0
Oct. 5........	+0.60	±0.26	2.9

1871

Date	Polar	Equatorial	P.−E. (arc)		Wt.
Apr. 21..............	39.0052	39.0314	−0″.63	±0.36	1.8
June 16..............	38.6212	38.6286	−0.18	±0.22	5.6
July 16..............	38.6382	38.6665	−0.68	±0.19	6.7
July 20..............	38.5628	38.5906	−0.67	±0.35	1.6
July 21..............	38.6286	38.6586	−0.72	±0.30	2.4
July 22..............	38.5671	38.6086	−1.00	±0.30	2.4
Aug. 12..............	38.7458	38.7440	+0.04	±0.21	7.1
Aug. 19..............	38.7322	38.7172	+0.36	±0.20	5.0

1872

Date	Polar	Equatorial	P.−E. (arc)		Wt.
May 7..............	38.2086	38.1963	+0″.30	±0.17	10.0
May 10..............	38.1743	38.1556	+0.45	±0.23	5.3
May 27..............	38.1213	38.1078	+0.32	±0.27	2.3
June 15..............	38.0063	37.9864	+0.48	±0.29	2.5
June 29..............	38.0341	38.0175	+0.40	±0.27	3.0
July 6..............	38.0121	37.9802	+0.77	±0.33	1.8
July 17..............	38.0575	38.0425	+0.36	±0.22	5.9
Aug. 10..............	38.1160	38.1249	−0.21	±0.14	14.3
Aug. 12..............	38.2410	38.2275	+0.32	±0.33	2.0
Sept. 21..............	38.5375	38.5132	+0.49	±0.30	2.5

In the above tables the first column gives the date on which the plate was taken; the second and third columns give the respective values of the polar and equatorial radii in scale divisions; the fourth column, the difference between the polar and equatorial radii in arc, together with the mean error of this result as determined from the separate measures; the fifth and last column gives the relative weights of the different plates as determined from their mean errors. The scale value differed for each plate, and was determined by assuming that the value of the mean radius of the Sun at distance unity is equal to 961″. Approximately one division of the scale is equal to 24″.6 of arc.

The different plates in each year give quite consistent results, but the mean results for the different years differ radically. The plates in 1871 show the equatorial radius to exceed the polar by some 0″.3; while the plates of 1870 and 1872, on the other hand, show the polar radius to be the greater by some 0″.2. Forming the mean by weights of the results obtained from the plates in the different years, we see that the yearly means are as follows:

1870, Sept. 22	+0″.50±0″.10
1871, July 19	−0.32±0.16
1872, July 2	+0.22±0.09

These measures thus seem to indicate a change in the relative sizes of the polar and equatorial radii of the Sun. During the interval 1871–72 the polar radius was increasing relatively to the equatorial, and by 1872 was decidedly the greater. These changes in the shape of the Sun are apparently real changes, and can hardly be accounted for in any other way. The plates were all taken with the same instrument and under the same conditions, and in corresponding seasons of the year. They were nearly all taken in the morning hours, and at approximately the same distance from the meridian. So far as can be determined from the data at hand, there is no instrumental explanation for the difference between the results in the different years.

The conclusion from this investigation is that during this period, 1870–72, there was a real change in the shape of the Sun; the equatorial diameter first increasing and then shrinking relatively to the polar diameter.

HELIOMETER MEASURES

While adjusting and determining the constants of the heliometers which were used in observing the transits of *Venus* in 1874 and 1882, the German observers made a great number of determinations of the Sun's diameter. In all some 2,692 separate measures of the diameter were made by twenty-three observers. Five heliometers were used, measures with the same instrument being made in various stations by the same observer, and in the same station by various observers. Thus Heliometer A was used by Adolph, Wittstein, Valentiner, Ambronne, Peters, Kobold, Deichmuller, Hartwig, Küstner, and Weinek in Strassburg; by Adolph and Valentiner in Tschifu; and by Franz and Kobold in Aiken.

This immense mass of data was most thoroughly discussed by Dr. Auwers in *Die Venus-Durchgänge, 1874 und 1882.* He reached the conclusion that the diameter of the Sun at distance unity is

$$1919''.26$$

and that the polar diameter exceeds the equatorial diameter by the amount

$$P.-E. = +0''.038 \pm 0''.023.$$

This apparent anomaly in the shape of the Sun was explained by Dr. Auwers as being due to the tendency on the part of an observer to measure vertical diameters greater than horizontal diameters. And this is quoted by Newcomb as conclusive evidence that the Sun is sensibly a sphere, and that there can be no non-symmetrical distribution of matter in the Sun sufficient to explain the anomalies in the motion of Mercury.

In forming his means, from which the above result was obtained, Auwers kept together all observations made with a single instrument, and thus observations of different years were grouped together. As arranged by Auwers, these observations do not afford any indication of a change of the relative diameters with the time. In order to investigate this point, I rearranged the series of observations, as given by Auwers, arranging them in order of the time without regard to the observer or the instrument. When thus arranged, the observations fall into two series: one extending from September 1873 to January 1875; the other, from May 1880 to June 1883. There is an isolated observation in July 1877, and another isolated one in March 1884. These do not fall into either series.

There is an uncertainty of some days in assigning a date to each determination of the ratio of the solar diameters; for the value of the difference between the polar and equatorial diameters (P.—E.), as given by Auwers for each observer, is found by him as the mean result of a number of observations, extending in many instances over a period of a month or more. In very few cases did an observer measure the polar and equatorial diameters on the same day, nor is the number of polar and equatorial diameters the same in any series. In reducing the observations of any one observer, Auwers took the mean of all diameters measured within 15° of the poles, and called such mean the polar diameter. He similarly took the mean of all diameters measured within 15° of the equator, and called such mean the equatorial diameter. The mean dates to which these mean diameters belong are not given by Auwers. For example, the observations made by Adolph in Strassburg were all made on fifteen days between September 2 and September 25, 1873. In this series Adolph made in all some fifty-seven determinations of the Sun's diameter; of which fifty-seven measures ten fall within the 15° limit of the pole, and nine within the corresponding limit of the equator. The polar measures were made on September 8, 14, 18, and 21; the equatorial measures, on September 18, 20, 21, 23, and 25. The remaining thirty-eight observations of this series were not utilized by Auwers in this investigation.

As a result of these nineteen measures by Adolph, Auwers finds the value for the ratio between the polar and equatorial diameters, $-0\overset{\prime\prime}{.}16$, in the sense Polar minus Equatorial; and this value, I have assumed, is the value for September 18, the mean date of the observations. Such an assumption is, of course, more or less approximate, but it gives a date sufficiently close for the purpose in hand.

SERIES OF 1873–75

In the first series of observations, extending from 1873 to 1875, there are in all thirteen such sets of observations. These are tabulated below, being arranged according to the mean dates of the observations; the weights being those assigned by Auwers.

TABLE I

Date	Observer	P.−E.	Weight
1873, September 18	Adolph	−0″.16	0.5
September 20	Borgen	+0.03	0.5
October 20	Valentiner	−0.21	0.5
December 15	Wittstein	+0.05	1.0
1874, February 4	Weinek	+0.15	0.8
March 18	Schur	+0.09	0.2
April 3	Adolph	−0.32	0.2
May 15	Schur	+0.15	1.0
December 26	Adolph	+0.16	1.7
December 28	Borgen	+0.21	0.1
December 29	Valentiner	+0.23	0.2
1875, January 5	Schur	+0.44	0.4
January 5	Seeliger	+0.13	0.2

While the separate determinations vary, a simple inspection of the above table shows that there was a progressive change in the difference between the polar and equatorial diameters. In the earlier measures the equatorial diameter was slightly the greater; in the later measures the polar diameter was decidedly the larger. This is shown not only by the average result, but by the measures of each observer. Adolph, Borgen, Valentiner, and Schur made observations in the fall of 1873 and the spring of 1874; again, these same observers made other series of observations in the latter part of 1874 and in January 1875. In the case of each of these four observers, the difference, P.—E., is greater in the latter series. These results are shown in the following table:

Observer	1873–74	1874–75
Adolph	−0″.20	+0″.16
Borgen	+0.03	+0.21
Valentiner	−0.21	+0.23
Schur	+0.14	+0.44

Again, divide the entire series of observations into three groups, placing in the first group all the observations made in 1873, in the second group those made in the spring of 1874, and in the third group those made in December 1874 and in January 1875. Give to each observer the weight assigned by Auwers, and form the weighted mean of each of the three groups. The observations then fall into the following order:

Mean Date	P.−E.	Weight
1873, October.	−0″.01	2.5
1874, March...........	+0.10	2.2
1874, December........	+0.21	2.6

And these means show the same progressive change as do the observations of the separate observers.

Thus these heliometer measures point toward a real change in the relative sizes of the polar and equatorial diameters of the Sun. It can hardly be doubted that this change is real, for it is shown by the observations of the individual observers, as well as by the observations of the different observers when grouped according to the time. Furthermore, this change is in the same direction as that indicated by the Rutherfurd plates made during the years 1871–72. The two series of measures, heliometer and photographic, thus supplement and confirm one another.

TABLE II

Date	Observer	P.−E.	Weight
1880, May 9............	Ambronn	+0″.19	1.1
July 15............	Ambronn	+0.08	4.0
1881, September 30............	Franz	−0.51	0.3
November 10............	Schur	+0.22	2.8
1882, March 14............	Kobold	+0.28	0.5
March 25............	Peter	+0.06	5.0
March 28............	Müller	+0.37	0.5
April 4............	Kobold	−0.05	2.8
April 6............	Marcuse	+0.07	0.7
April 15............	Küstner	.00	0.4
April 15............	Kempf	+0.09	1.7
May 3............	Deichmüller	−0.04	4.0
May 15............	Hartwig	+0.09	6.2
May 15............	Schur	+0.13	4.0
May 22............	Franz	+0.06	0.5
June 4............	Wislicenus	+0.02	4.0
July 2............	Bauschinger	−0.05	1.7
November 20............	Franz	−0.06	1.0
November 25............	Küstner	+0.13	1.3
November 25............	Kobold	−0.01	1.7
November 30............	Deichmüller	−0.30	0.1
November 30............	Müller	−0.34	0.2
November 30............	Auwers	−0.01	1.7
December 2............	Wislicenus	−0.28	0.3
December 4............	Peter	+0.15	0.5
December 5............	Kempf	+0.32	0.6
December 5............	Hartwig	+0.30	0.7
1883 May 15............	Wislicenus	−0.21	6.2
June 4............	Hartwig	−0.02	2.8

SERIES OF 1880-83

In this series there are in all twenty-nine sets of observations, of which number, however, twenty-three were made in 1882. These are tabulated in Table II, being arranged according to the date of observation in a manner entirely similar to the former table for 1873-74.

An inspection of these results will again show a change in the difference between the polar and equatorial diameters. This change is not at once apparent, for the relative weights of the separate determinations in this series differ greatly, the largest being 6.2, the smallest 0.1. Some of the determinations of small weight differ considerably from adjacent and better observations, and these poor values tend to conceal the progressive change in the ratio between the diameters. This change, however, is clearly brought out when the observations are divided into groups and the weighted means of each group formed. When this is done, we find that the observations arrange themselves as in the following table:

Date	P.–E.	Weight
1880, June	+0.″10	5.1
1881, October	+0.15	3.1
1882, March	+0.06	11.6
June	+0.05	20.4
November	+0.05	8.1
1883, May	−0.15	9.0

We thus see that during the interval from 1881 to 1883 there is a progressive change; the equatorial diameter apparently growing longer in relation to the polar diameter. While the division of the observations of the year 1882 into three groups is more or less arbitrary, yet, no matter how these observations had been grouped, the progressive character of the change would have been apparent. The mean of all the determinations for the year 1882 is +0.″05 with a weight of 40.1.

The change thus found for the period 1880-83 is the reverse of that found for the former period 1874-75, at which time the equatorial diameter was found to be growing shorter relatively to the polar diameter.

NORTHFIELD PLATES

Under the direction of Professor W. W. Payne, Dr. H. C. Wilson has taken a long series of solar photographs at Northfield, Minn. Only a few of these photographs are available for measurement; most of the plates lack satisfactory orientation, and in many the edge of the image is blurred, owing to a defective shutter. Dr. Wilson selected and sent to Columbia University for measurement five plates, which were taken during the years 1893 and 1894, all of which were well oriented and had on them the necessary data for measurement and reduction.

These five plates were measured in the same manner as were the Rutherfurd plates, with the following results:

1893

Date	P.-E. (arc)		Wt.
Sept. 8.........	−1'.10	±0'.24	2.8
Sept. 9.........	−0.94	±0.21	3.7
Sept. 11........	−0.72	±0.18	5.9

1894

Date	P.-E. (arc)		Wt.
July 10.........	−0'.72	±0'.24	2.8
July 17.........	+0.36	±0.23	3.1

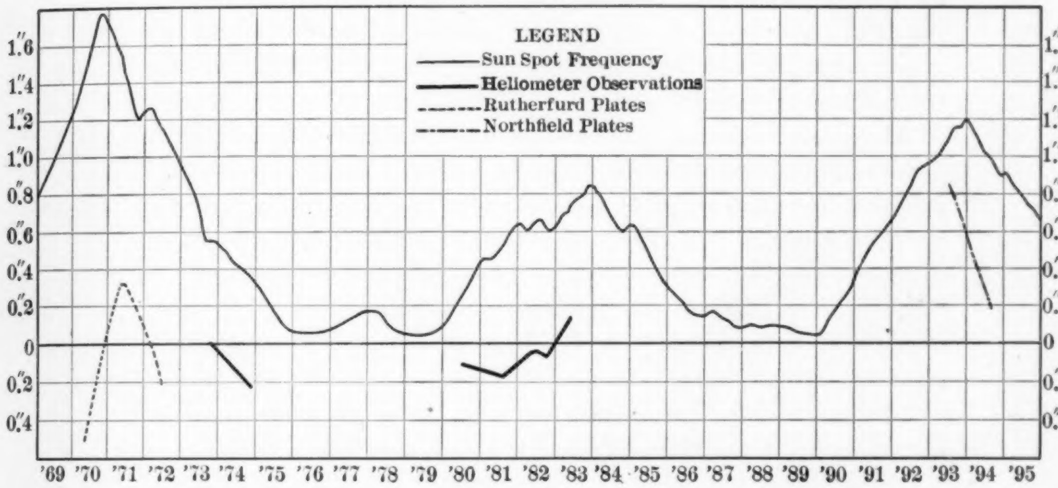
Forming the means by weight we find for the yearly means:

1893, September 10 - - - - - - −0'.87±0'.10
1894, July 13 - - - - - - −0.20±0.23

These measures again indicate a change in the ratio between the polar and equatorial radii. The plates are too few in number to give any conclusive result, but they seem to point toward a shrinkage of the equatorial with respect to the polar diameter. By themselves these plates would have but little weight in forming any conclusion, but taken in conjunction with the Rutherfurd plates and the heliometer measures, they confirm the general result, that the shape of the sun is variable.

RELATION OF THE SUN'S FIGURE TO THE NUMBER OF SUN-SPOTS

A sun-spot maximum occurred in the latter part of 1870, and from this time until 1876 there was a steady diminution in the number of spots. In 1870 and 1871, just previous to the maximum, the Rutherfurd plates show that the equatorial diameter was increasing, but during the period 1871 to 1876 the Rutherfurd photographs and the heliometer measures both show that the equatorial diameter was shrinking relatively to the polar diameter. The period from 1880 to 1883 was a period when the number of sun-spots was increasing, the sun-spot maximum occurring in the latter part of 1883. During this period the heliometer measures indicate that the equatorial diameter was increasing relatively to the polar diameter. But the 1883 sun-spot maximum was hardly half as high as the maximum in 1870, and we should expect, therefore, to find the changes in the Sun's diameter less marked in later period than in that of 1870. This is exactly what the determinations show. A third sun-spot maximum occurred in the latter part of 1893, and during 1894 the number of spots rapidly decreased. The Northfield plates show that during this period the equatorial radius was decreasing relatively to the polar radius.



RELATION OF THE SUN'S FIGURE TO THE NUMBER OF SUN-SPOTS

This relation between the frequency of sun-spots and the figure of the Sun is shown in the accompanying diagram. The curve of sun-spot frequency is taken from Miss Clerke's *Problems in Astrophysics.* The dotted curve represents the relation between the equatorial and polar radii of the Sun, as deduced from the Rutherfurd plates; the full heavy curves, the changes as exhibited by the heliometer measures, and the broken line, the changes as indicated by the Northfield plates. These curves are plotted from the weighted means of the observations, as given in the above tables, but with the signs reversed, so that the portions of the curves above the zero line represent those observations which show the equatorial diameter to exceed the polar. The slopes of the observational curves are nearly parallel to the corresponding portions of the sun-spot curve, and the general character of these curves shows a striking resemblance to the curve of sun-spot frequency.

The present investigation would seem to show, therefore, that the ratio between the polar and equatorial radii of the Sun is variable, and that the period of this variability is the same as the sun-spot period. The Sun appears to be a vibrating body whose equatorial diameter, on the average, slightly exceeds the polar diameter. At times, however, the polar diameter becomes equal to and even greater than the equatorial—the Sun thus passing from an oblate to a prolate spheroid.

In this variable figure of the Sun may lie the explanation of the anomalies in the motions of *Mercury*, *Venus*, and *Mars*.

Columbia University Observatory,
New York City,
June, 1905.

THE ORBIT OF THE SPECTROSCOPIC BINARY ζ *TAURI*

By WALTER S. ADAMS

The star ζ *Tauri* was included in a list of stars with variable radial velocities published by Professor Frost and the writer in 1903.[1] Attention was called at that time to the peculiar character of its spectrum, and because of the interest attaching to it for this reason, as well as on account of its comparatively long period, it was observed by us with considerable regularity. Previous to my leaving the Yerkes Observatory, twenty-two plates had been obtained, and I am indebted to Professor Frost for three others, secured since that time, which he has kindly placed at my disposal. These have proved of great value in the determination of the star's period.

The general features of the spectrum in the region covered by the plates may be described briefly as follows: A strong, well-defined line of considerable breadth at $H\gamma$; very weak and diffuse helium lines at λ4388 and λ4472; a similar, though slightly stronger line due to magnesium at λ4481; and, finally, a number of faint broad lines identical for the most part with the enhanced lines of iron and titanium.

The question at once arose, in connection with the measurement of the plates, whether increased accuracy would be attained by the use of any lines in addition to $H\gamma$, and a few measures were made on the basis of assigning weights to the various lines at the time of measurement. On account of the extremely high relative value of $H\gamma$, however, this plan did not prove satisfactory, and it seemed best finally to base the determinations upon that line alone. As the width of $H\gamma$ is considerable, amounting in the average to about 0.75 tenth-meters, which would correspond to 52 kilometers in velocity, duplicate measures have been made throughout with a view to reducing the accidental errors of setting upon the line. With one exception, these have agreed reasonably well for all of the plates. This was in the case of the first plate of the series, B219; the first measurement gave a value of +17.6 km; the second, a value of +5.9 km. It was evident that a totally different estimate of the position of the

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[1] *Astrophysical Journal*, **17**, 150–153, 1903.

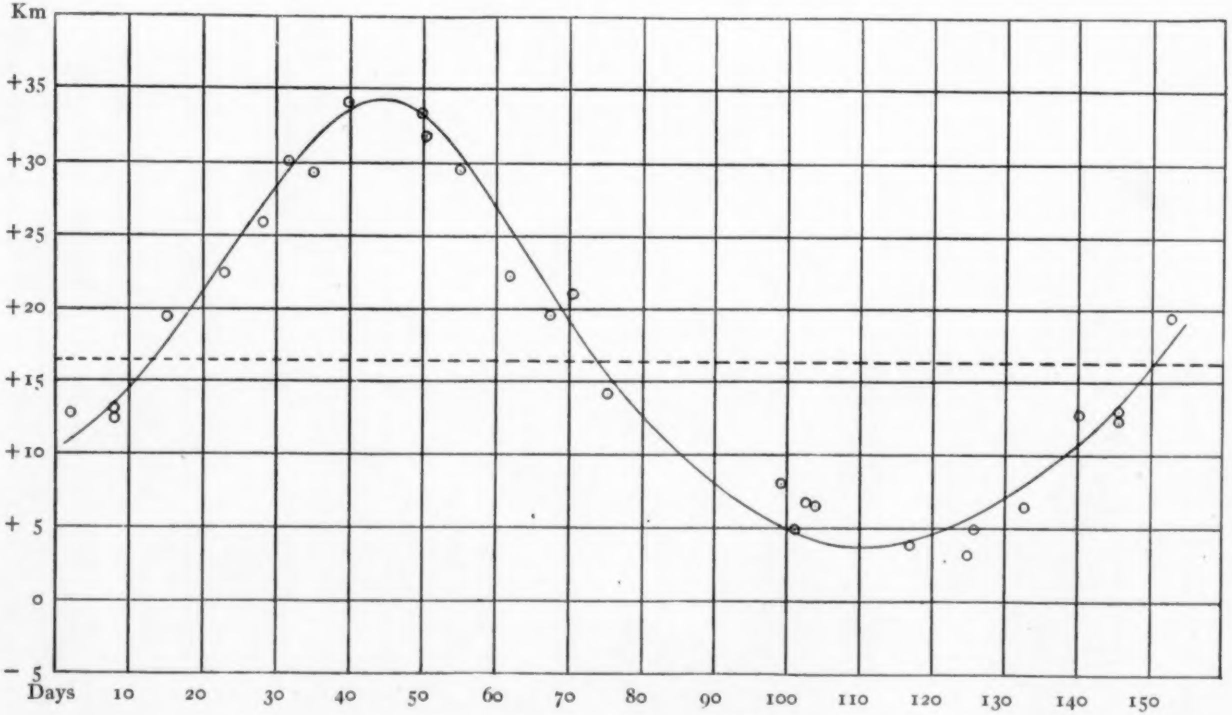
line had been made in the two cases, and, as neither the comparison nor the stellar spectrum upon this plate is satisfactory, it has been omitted in the discussion.

The table which follows gives a summary of the material used in the determination of the orbit, including the values given by the separate measurements as well as their mean. As in previous communications, the series letters A and B refer to the cameras employed.

Plate Number	Date, G. M. T.	First Measure	Second Measure	Mean	O.-C.
		km	km	km	km
A 317	1902, February 12.7	+23.0	+19.2	+21.1	+2.2
B 332	April 23.6	14.7	11.1	12.8	+1.8
B 410	September 13.8	13.2	13.3	13.2	−0.4
B 425	October 15.8	33.8	34.1	34.0	+0.7
B 440	October 30.8	31.2	28.0	29.6	−1.0
B 452	November 6.9	19.4	25.2	22.3	−3.1
B 462	November 19.9	13.5	14.8	14.2	−1.4
B 470	December 17.8	6.5	7.1	6.8	+2.2
B 473	December 18.8	6.7	5.9	6.3	+1.8
B 476	December 31.8	4.0	3.8	3.9	−0.5
B 482	1903, January 8.8	2.3	4.1	3.2	−2.4
B 485	January 9.7	4.5	5.4	4.9	−0.9
A 386	January 16.8	4.4	8.7	6.5	−1.3
B 489	January 29.8	14.3	10.4	12.4	−1.2
A 397	February 5.8	18.7	20.3	19.5	+1.7
A 403	February 13.7	22.5	22.2	22.4	−0.8
A 407	February 18.7	26.3	25.5	25.9	−0.9
A 412	February 26.5	28.9	29.7	29.3	−2.3
A 420	March 13.7	31.4	31.9	31.7	−1.0
A 447	April 30.6	7.9	8.5	8.2	+2.9
B 533	December 13.6	34.1	32.2	33.2	+0.1
A 494	1905, January 13.7	30.6	29.8	30.2	+0.8
B 571	February 18.6	19.7	19.5	19.6	−1.4
B 582	March 24.6	5.0	4.9	4.9	+0.1

Plate B 452 is underexposed, and the considerable difference between the values given by the two measurements is probably due to this fact.

A brief inspection of these results showed the period to lie in the vicinity of four and one-half months, and a few trials gave a value of 138 days as most consistent with the observations. The latter were then plotted with this period, and a smooth curve drawn through them. The departure of this from a sine curve made it evident that the eccentricity must be considerable, and accordingly the graphical method of Lehmann-Filhés was adopted for the computation of the orbit.



VELOCITY-CURVE OF ζ *TAURI*

With the use of a planimeter to adjust the areas, the following quantities were obtained as a basis from which to derive the elements:

$$\text{Velocity of system } V = +16.4\,\text{km}$$

$$A = 17.6\,\text{km};\ B = 12.3\,\text{km};\ z_1 = +4.23;\ z_2 = -4.50\ .$$

The notation is that of Lehmann-Filhés.

These quantities gave the following elements:

$$u_1 = 100^\circ\ 13'$$

$$\omega = 9^\circ\ 45'$$

$$e = 0.180$$

$$T = 1902,\ \text{January } 19.9$$

$$a \sin i = 27{,}900{,}000\,\text{km}$$

$$\text{Period } U = 138 \text{ days}$$

$$\mu = 2^\circ\!.609$$

The time of periastron passage T nearly coincides with the time of maximum radial velocity, which shows that the major axis of the orbit must be nearly perpendicular to the line of sight.

The velocities for the dates of observation were computed from these elements, and the differences between the observed and the computed values are given in the column O.-C. of the table above. The largest residual is - 3.1 km, a result which is quite satisfactory in view of the fact that the determinations are based upon only one line. The quantity of material at present available for discussion does not seem to the writer sufficient to make a least squares solution of value.

The accompanying plate shows the velocity-curve derived from the set of elements, and the positions of the observed velocities in reference to it. The broken line drawn parallel to the axis of abscissas is at a distance of 16.4 above it, which is the velocity in kilometers of the center of gravity of the system.

No trace of the spectrum of the second component has been found upon any of the plates, the evidence afforded by the remarkably well-defined character of $H\gamma$ being especially conclusive.

SOLAR OBSERVATORY,
Mount Wilson,
June 1, 1905

ON THE MAGNESIUM SPARK

By W. W. STRONG

In 1902[1] Dr. Mohler used the shift of the spark lines to measure the velocity of the particles driven off from the electrodes. He found a probable velocity of about 400 meters per second for certain iron, aluminum, magnesium, and cadmium lines. While getting this Doppler effect, it was found that the character of the magnesium lines when photographed "end-on" was different from that of the same lines when the spark-gap was revolved 180°. Using magnesium and iron electrodes, Dr. Mohler found that when the spark-gap was "end-on" and the magnesium electrode was next the slit, the lines $\lambda\lambda 2795$, 2802, and 2852 would be strongly reversed, like $\lambda 2852$ in the arc. If, however, the iron electrode was nearest the slit, these same lines would be without reversal. The purpose of this investigation was to find the cause of this reversal.

The conditions under which the work was done were the same as those under which Dr. Mohler worked.[1] A concave 4-inch Rowland grating, ruled 14,400 lines per inch, was used for photographing the spectra. A 110-volt current with a Wehnelt break was used in the primary circuit. The Rühmkorff coil used was capable of giving an 18 cm spark. The capacity used in the secondary circuit was usually a small Leyden jar of 0.0025 microfarads capacity, and the maximum length of spark was about 1.5 cm, this length varying with the size of the platinum electrode exposed in the solution of the Wehnelt break. The usual length of spark-gap was from 3 to 8 mm. A resistance box was used in the primary circuit, and the resistance was changed to suit the break. The work was done in the second and third order of spectra, and all photographing was done in a basement where the temperature was very constant and the spectroscope was free from jars.

It was found that when magnesium electrodes were used, the "principal series" lines $\lambda\lambda$ 2802 and 2795, with the line λ 2852, would be widely reversed for the two "end-on" positions, for the vertical

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[1] *Astrophysical Journal*, **15**, 125, 1902.

and transverse positions. The "first subordinate series" lines λλ2798 and 2791, were usually reversed also, but the width of their reversals was much smaller. The line λ 2779, the middle line of the quintuple group, was sometimes reversed. The "second subordinate series" lines, λλ2936 and 2928, and the line λ4481, were never reversed. When there was a heavy current in the primary circuit, reversal did not always occur. Spectrograms of the edges of a vertical spark-gap showed the lines λλ4481, 2936, and 2928 somewhat weakened.

If either iron, copper, or zinc were substituted for one of the magnesium electrodes, and the magnesium electrode was away from the slit of the spectroscope, the lines λλ2852, 2802, 2798, 2795, and 2779 would be without reversal. When the spark-gap was revolved 180° and the magnesium electrode was next the slit, these lines were reversed just as they were when both electrodes were of magnesium. As a rule, iron was substituted for one of the magnesium electrodes on account of the abundance of iron lines in this region. When such a spark-gap was vertical, the width of the reversed parts of the above magnesium lines was less than for the "end-on" position. For the "across" position ("end-on" position revolved 90°), sections made near the magnesium electrode showed reversal; whereas sections made near the iron electrode gave less or no reversal. The following table gives the results as found for the "end-on" positions:

Wave-length of magnesium lines....	4481	2936	2928	2852	2802	2798	2795	2791	2779
Width of reversal, *Mg* next the slit..	0	0	0	0.2	0.24	0.15	0.27	0.15	0.1
Width of lines, *Mg* next the slit.....	1.5	0.6	0.5	0.6	0.90	0.60	1.00	0.60	0.3
Width of reversal, iron next the slit..	0	0	0	0	0	0	0	0	0
Width of lines, iron next the slit....	1.3	0.6	0.5	0.3	0.45	0.40	0.45	0.43	0.3

It will be seen from the above table that the lines λλ2852, 2802, and 2795, the ones that are most reversed when the magnesium electrode is next the slit, decrease very considerably in width when the iron electrode is next the slit, while the other lines remain of the same width, especially the lines λλ 2936 and 2928. It should be remembered that the above figures are only rough approximations.

The above results would seem to indicate that there is a kind of an enveloping "reversing" layer of magnesium particles around the spark, and that this layer or sheath does not extend across the spark-gap as far as the inner portion of magnesium particles, or at least that it is much thinner near the iron electrode. That the spark should be differentiated into different layers does not seem remarkable, since the arc itself, as shown by Lockyer,[1] Baldwin,[2] and Foley,[3] is composed of different layers which emit different spectra. The lines that showed greatest reversal, λλ 2852, 2802, and 2795, are also reversed in the arc. Henry Crew[4] finds that the width of the reversal of these lines and λ2779 is "greatly increased" in an atmosphere of hydrogen. A. S. King[5] finds that self-induction very much reduces the lines λλ 2791, 2795, 2798, and 2802 in the spark, while λ2852 is made blacker and the reversal is narrowed. All this would seem to indicate that the phenomenon is due to absorption by the relatively cool vapors of magnesium, and that the lines λλ 4481, 2936, and 2928, are not thus subject to absorption. For if one accepts the view that hydrogen reduces the temperature of the arc, one would expect a greater amount of absorption. Also, in a spark with capacity in the circuit, the discharge is oscillatory, and the series of sparks passing back and forth in quick succession tend to change the spark into an arc. Now, self-induction[6] "increases the time the oscillations persist, and so enables the vapor of the metal to get well diffused through the spark-gap." One would then expect the lines to be much less broadened, and hence not reversed. To prove this an absorption effect, a vertical spark-gap (*Mg*1, *Mg*2, Fig. 1) of magnesium electrodes, was placed between the slit and an "end-on" spark-gap (*Fe*, *Mg* 3) of iron and magnesium, with the iron electrode nearer the slit. Now the unreversed lines from the iron-magnesium spark-gap should be reversed by the double magnesium spark-gap. Reversal was found to occur.

During the work, efforts were made to get a Doppler effect. The quintuple group showed a slight shift which was hardly within the limits of measurement. Two "end-on" spark-gaps of magnesium electrodes, the sparks being made to go in opposite directions, were photographed together. Such a spectrogram should show twice

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[1] *Proc. R. S.*, **28**, 425, 1879.

[2] *Physical Review*, **3**, 370, 448, 1896.

[3] *Ibid.*, **5**, 129, 1897.

[4] *Astrophysical Journal*, **12**, 167, 1900.

[5] *Ibid.*, **19**, 225, 1904.

[6] J. J. Thomson, *Conduction of Electricity through Gases*, p. 396.

the Doppler effect on one exposure, but the lines were so diffuse that measurement was impossible. It would be very interesting to find whether the reversed lines would show the same shift as the other unreversed lines. It may be that the different values found for different lines of the same element is due to the fact that these lines are produced by different parts of the spark.

To try further the reversal effect, the spark was made to pass through a fine hole. For holes less than 0.5 mm in diameter the "reversing" layer was entirely cut off. Spectrograms taken of the spark between the hole and the iron electrode never gave any reversal. For these small holes very long exposures were necessary, as very little magnesium came through the hole. All lines are of course faint, but the lines λλ 2928 and 2936 do not appear at all through a hole 0.16 mm in diameter; while λ 4481 is very faint, it usually occurring as the strongest line of all. Only the arc lines remain, and their relative intensity is the same as in the arc. Further work on this point is intended.

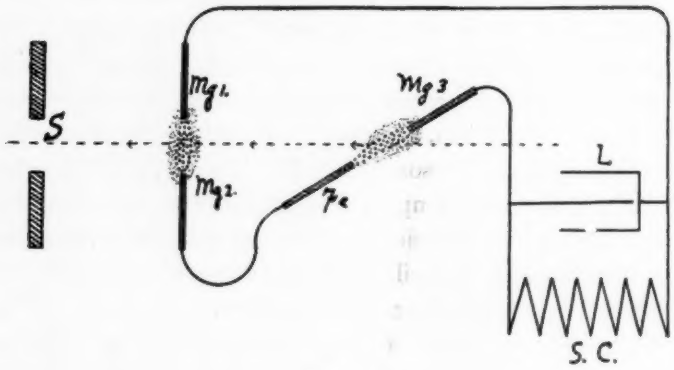


Fig. 1

In conclusion, the writer wishes to thank Dr. Mohler for the suggestions and aid which he has given, and also for his constant kindness and courtesy.

Dickinson College,
Carlisle, Pa.,
May 1, 1905.

DIFFRACTION GRATING REPLICAS

By ROBERT JAMES WALLACE

In the early part of 1901 the writer entered upon a series of experiments having for their object the duplication of the plane grating, with the idea of producing a method which would yield definitely reliable results under conditions which might be easily satisfied. As these experiments have continued at intervals since then up to the present time, and have resulted in the manufacture of replica gratings of high grade, which are being widely used, it seems advantageous that a description of the method employed in their manufacture should be placed on record, for the guidance of those interested.

The publication of these details has been purposely delayed in order that sufficient time should elapse to preclude the possibility of deterioration; and also that opportunity might be afforded for the collection of data relative to their behavior. As both of those points have been answered in a satisfactory manner, there is therefore no further reason for delay.

The superior suitability of a transparent grating for a very considerable amount of work is of course evident, while the ease and certainty of production renders it possible that gratings need not be (as at present) excluded from high schools and kindred institutions on account of their cost. Apparatus may be constructed, or experiments undertaken, which would not be deemed advisable if one had to risk an original grating, while the duplication of gratings giving abnormal spectra is rendered not only possible, but easy.

It seems unnecessary that we should here enter upon a résumé of the various endeavors which have been made by earlier workers in this direction, beginning with Strutt in 1872 and continued by him as Lord Rayleigh in 1896 and down to the present time. However, there is one name which should not be lightly passed by in this consideration, for Thorp, of Manchester, England, was the first worker to produce a really presentable grating-duplicate of considerable efficiency, and there is certainly owing to this worker from the scientific world a decided debt of gratitude.

Mention may also be made of still another effort which is subsequent to that of the author. Mr. F. E. Ives, of New York, after receiving a few replicas from the writer became interested in the subject and himself undertook the problem of making a successful replica. After a series of experiments, he succeeded in producing a cast which differed only in respect to its method of mounting. Owing to the fact that application has been made for patent, complete details of the process are not available, but it is sufficient to state that claim is made for a replica in "a harder and less elastic material than celluloid," and with a smaller contraction coefficient. This is pressed face down in contact with a piece of selected plate glass, and then covered by and cemented to another similar plate with a cement whose refractive index is the same as that of the cast. The writer was honored by the receipt of one of these "new process replicas" upon their introduction about the beginning of the present year. When tested upon the spectrometer, it gave very good results—comparable with those manufactured by the method about to be described. Unfortunately, these casts do not seem to be permanent, as the cementing medium appears to be a solvent of the film, so that now diffraction colors are only to be seen in isolated patches.

Thorp's method[1] consisted in flowing the original grating with a thin film of high-grade oil, upon which was poured celluloid in solution. When dry, this was peeled from the previously oiled surface and mounted face up on a plate of glass by means of a solution of gelatine and glycerine; the film being lowered gently and gradually into contact.

In all of the Thorp grating casts a very large number of air-bubbles are evident between the grating film and the glass support, the presence of which serves to scatter the light and impair the brilliancy and definition of the spectrum. In the method of mounting employed by the writer these air-bubbles are entirely eliminated, the replicas presenting a clean and brilliant appearance.

When in 1901 the matter of casting from the Rowland's grating was begun, the method employed was that indicated by Thorp, viz., celluloid. A solution was made of gun cotton in amyl acetate, and then camphor was added in sufficient quantity both with and without

[1] Patented in England.

the addition of alcohol. Innumerable difficulties were encountered which, when suppressed or surmounted, simply gave place to others; and although considerable experimental work was performed, it but served to show the unreliability of this solution. These difficulties lay mostly in the direction of uneven shrinkage and opalescence of the film; while gratings of quality sufficiently good to define well in the spectroscope constituted only about 20 per cent. of the entire number.

In 1902 the results of further experiment led to the discontinuance of the preliminary coating with oil, and the exclusion of camphor in the solution. This change (together with an alteration in the method of stripping and mounting) resulted in much greater success in the production of replicas of a high grade, giving also a decidedly more brilliant film. This solution (which has since been in use without change) is composed of

Amyl acetate, pure (Mallinckrodt)	64 cu. cm (2¼ oz.)
Anthony's snowy cotton	2.5 grams (38 grains)

The cotton should be added to the amyl acetate in small quantities at a time, and well shaken until dissolved, after which it is allowed to stand during twenty-four hours. At the end of that time the resultant collodion is precipitated by being poured in a very thin stream into a large tray filled with water. The collodion should be poured from a height of at least three or four feet, and the water meanwhile should be constantly stirred with a glass rod. The precipitation does not immediately occur, the collodion collecting in an oily scum upon the surface of the water, which must be stirred from time to time during the course of the ensuing twenty-four hours.

When precipitation is complete, it will present the appearance of white or very light gray flocculent masses, which float upon the surface of the water, and are collected upon a clean filter paper and set aside to dry.

When thoroughly dry, it is again dissolved in the following proportions:

Amyl acetate, pure (Mallinckrodt)	64 cu. cm (2¼ oz.)
Precipitated cotton	2.5 grams (38 grains)

and the collodion carefully filtered through paper—a process which may be advantageously hastened by the use of an aspirator or other form of air-pump.

The writer has prepared and used this collodion both with and without precipitation, but preference is given to the former as producing a film which is not only more brilliant, but has a much more even and regular shrinkage in the stripping.

The grating to be duplicated is first carefully leveled in a roomy drying-cabinet, and, after dusting with a soft camel-hair brush, the necessary amount of solution is flowed over the face. The exact quantity lies within wide limits; too small an amount produces a film so thin that one has difficulty in handling it, while too much gives a film which dries with a more or less matt surface. By using always the same container one may drop the necessary quantity, and then by inclining the grating cause it to flow over the surface. From the container used by the writer twenty-five drops is the average amount for a two-inch grating.

It seems hardly needful to indicate that this flowing of the grating should be performed in an atmosphere as free from air-currents as possible, thus minimizing the danger of dust particles settling upon the surface during the operation. The grating is then placed upon the leveled support in the drying-cabinet, and the door carefully closed.

The drying is rather slow, a two-inch grating taking about eight to twelve hours, but it cannot be advantageously hurried. In the opinion of the writer, the slower the drying, the better the result, as the solution gets time to fill perfectly the minute grooves made by the cutting diamond. It is a notable fact that casts made from collodions of different composition, and drying quicker, did in no case give results which were at all comparable with those which had been obtained by the slower method. It has also been noted that this film may be advantageously left in contact with the original for a considerable length of time, up to about three or four days, as it is much more easily handled in the process of stripping and mounting.

After an extended trial of various mounting mediums, which need not be enumerated, preference was given to a very thin layer of plain hard gelatine (with which the glass is previously coated and dried on a leveling-slab). The mounting is performed in the following manner:

The gelatine-coated glass and the thoroughly dry grating are placed (face up) in a tray containing filtered distilled water at normal temperature, and the tray covered over with a clean glass. After a few minutes the extreme edges of the ruling will begin to show shadow bands caused by the contraction of the film, and thus pulling the lines "out of step." When this is observed, the grating should be removed from the water, and any adhering globules shaken from its face; then by a gentle pressure of the thumb nail at the edge of the clear portion of the polished circle, the film will be caused to spring apart from the original. This loosened portion is then grasped by the blades of a pair of wide "cover-glass" forceps, and with an even, slow motion raised from the original in a direction parallel to the length of the ruled line. *Immediately* the film is free it is laid face up upon the gelatine-coated plate (which is removed from the water for that purpose) in the same manner as in lowering a cover-glass upon a microscope slide; the plate is tilted to drain it of superfluous water, and the edge of the replica is clamped in contact, by means of a wide spring "letter clip" with matched edges. A piece of the softest velvet rubber, with a carefully cut edge, is now drawn *very lightly* and evenly over the replica in the same direction as in stripping, viz., parallel to the line length, and the plate set aside to dry.

This entire operation of stripping and mounting is very rapidly performed, and although the description may appear lengthy in the recital, it can only be laid to the fault of the author.

It has also been found advantageous to "ring" the replica with the casting solution after it has dried, and thus prevent the separation of the replica under extreme hygrometric conditions.

The contraction of the film during the process of mounting alters the number of lines to the inch, but such shrinkage is very small and is easily controlled within limits, viz., length of drying time. In those manufactured by the writer, which have a drying time of twenty-four hours, this contraction has been determined by careful measurement of over thirty replicas, with the following result:

$$\left.\begin{array}{l}\text{Width of original ruling, 28.867 mm}\\ \text{Width of replica ruling, 28.691 mm}\end{array}\right\}\text{ Mean of 10 settings each,}$$

which gives a difference of 0.176 mm on the entire width of ruled surface.

The total number of lines on the original is 16,397; hence 568 = 1.0 mm. On the replica the total number of lines divided by the width gives the new constant, viz., 572 nearly, or an increase of about six lines to each one thousand.

This contraction of the replica, and the consequent increase in the number of lines to the mm, result in a greater dispersion of the spectrum. In a photograph of the region between $\lambda 3933$ and $\lambda 4308$, with the original ruling, the separation of the lines K and G was found to be 27.68 mm, as against 28.12 mm on the negative taken through the replica.

An enlargement of these negatives is shown in Plate III, which illustrates the capability of the cast under similar conditions with the original, taken with a spectrometer having an aperture of 25.0 mm and a camera focal length of 300 mm. Examination of the original negatives under a glass shows that everything which is resolved with the original grating is equally well shown in the negative from the replica (which was not especially selected for this purpose, but was taken indiscriminately from the stock of "First quality" replicas).

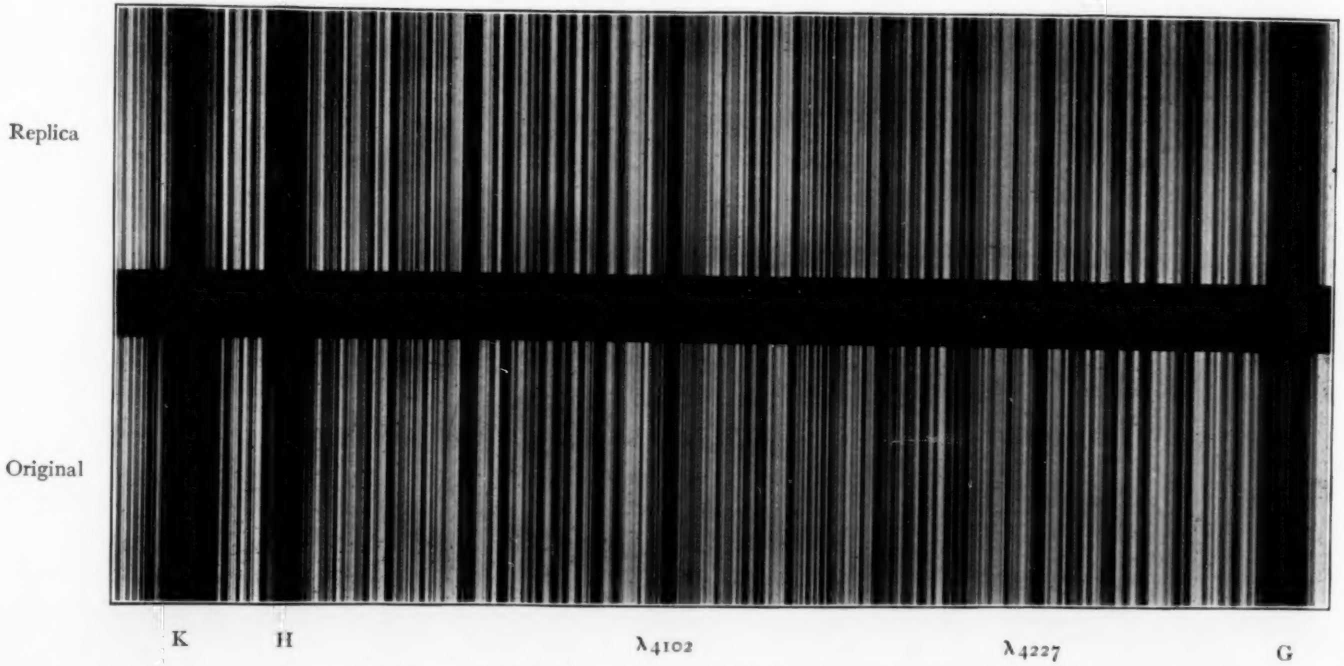
The quality of glass upon which the replica is mounted has much to do with its efficient performance in the spectroscope. It is not essential that one use worked flats, but it is necessary that the surfaces be of fairly good quality; the glass in use by the writer is "white optical crown" which has been reground and polished, and which may be graded by preliminary observation in a spectroscope.

Not all casts are of first quality or give equally good definition, for, while under apparently identical conditions of manufacture, the results vary. This is undoubtedly due to the "personal equation" in the process of mounting, and for this reason they are tested and graded. Those of "second quality" are useful for projection purposes, and also in the chemical laboratory for the flame test of *K*, *Na*, etc.

Not every sample of gun cotton will give an equally good film, and from many varieties tested by the writer the brand before specified was with much care selected as the best—not only on account of the smoothness of the resultant film, but principally because it was found to be entirely free from any trace of acid. It will, of course, be evident that if this were not the case, it would only be a question of the number of casts made, which would determine the life of the original grating.

If a replica grating be superposed face down upon the original ruling, and observed at an angle which equals that formed by the

PLATE III



Solar Spectrum as Photographed with a Grating and its Replica
Aperture 25 mm; 15,000 lines to the inch

incident light, a series of more or less symmetrical shadow bands will be noted, which run approximately parallel with the ruling, and are caused by interference on account of the slight difference in the line spacing. If the replica were ***absolutely*** perfect, the bands would be straight, but in general they are slightly curved, such curvature forming (at the grating edge) an arc of a circle of radius approximately 1.5 meters. In but few cases have they come closer to a straight line than this, which may be considered as a fair average. The counting of these shadow bands was the method used by Thorp in determining the actual number of lines per inch in his casts—their number corresponding to the increase by contraction.

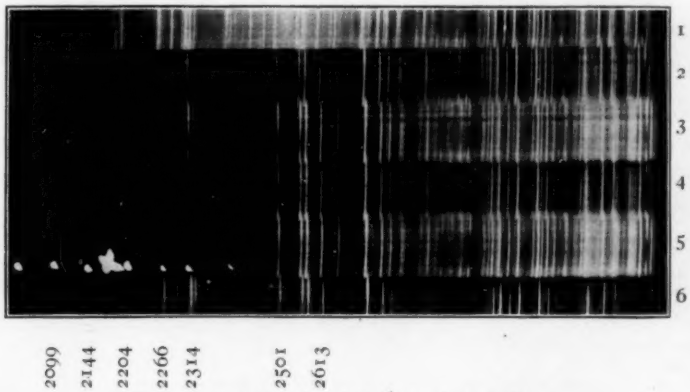


FIG. 1.—Spectra of cadmium-lead-zinc alloy.

No. 1—No screen.
No. 2—Thin mica screen.
No. 3—Same (with longer exposure).
No. 4—Grating film very thick.
No. 5—Same (with longer exposure).
No. 6—No screen, shorter exposure than No. 1.

On examination of these films in a quartz spectrograph, to determine their power of transmission for ultra-violet light, it was found that the absorption was practically *nil* up to λ2613, and with slightly longer exposure they would transmit up to λ2314. As glass practically stops everything beyond λ3400, it has been suggested by Professor R. W. Wood (to whose kindness the writer is indebted for this examination) that they be mounted upon selected thin sheets of mica, which are even more transparent to light of shorter wave-length. Fig. 1 shows the photographic record[1] of this examination.

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[1] From negative by Professor R. W. Wood with quartz spectrograph of the Johns Hopkins University.

For special apparatus these casts may be readily mounted in a variety of ways suitable to the end in view, while for direct vision they may be mounted upon 30° prisms of light crown. This method is very suitable in the construction of small spectroscopes, micro-spectroscopes, etc., and largely eliminates the possibility of poor definition due to multiple reflection from the faces of the glass plate when not accurately parallel.

In conclusion, it may be remarked that the process appears to be absolutely devoid of any evil effect upon the original grating; in fact, the opposite is strictly the case, for concerning a one-inch grating owned by the writer, from which upward of one thousand replicas have been taken, the surface appears to be as brilliant as when newly ruled. In cases where gratings have been in use in class instruction for a number of years, and consequently present a very bad appearance, being dull, surface-scratched, and greasy, the making of a number of successive casts restores in a great degree the original brilliancy. The explanation is obvious: the "dirt" on the grating is imbedded in the film while fluid, and, "setting" therein, is removed with the cast. A series of such casts from a dirty grating presents a good object-lesson as to the efficiency of this method of cleaning, being much superior to the means usually employed, viz., alcohol, ether, ammonia, etc. The replicas themselves are sufficiently tough to bear careful washing, their elasticity allowing them to be rubbed with cotton without injury.

Methods are now under consideration for giving a suitable reflecting surface to these casts, so that they may be produced either in the form of transmission or reflection gratings. Numerous experiments have also been tried in an attempt to duplicate the concave grating, but these, up to the present writing, have not given a sufficient measure of success to warrant their being recorded here.

YERKES OBSERVATORY,
May 15, 1905.

THE SPECTRA OF ALLOYS

By P. G. NUTTING

In connection with the general problem of the relative intensity of two spectra coming from the same source at the same time, a study of the spectra of alloys is of importance, and in practice, if working with arc or spark spectra with impure electrodes, it is desirable to know the probable influence of this impurity on the spectrum worked with, and by what means this influence may be miminized. Or, if the electrodes are alloys of nearly equal parts of two metals, is it not possible, by varying the current, capacity, inductance, or atmosphere, to cause the spectrum of either component metal to preponderate at will? While spectroscopic phenomena connected with the spark are extremely complex in detail, effects involving spectra as a whole are not difficult to observe.

The only previous work on the spectra of alloys appears to be that of Lockyer and Roberts.[1] They showed that the so-called "long" lines were the first lines to appear in the spectrum of an impurity as the proportion of that impurity was gradually increased. They even sought to build up a spectroscopic method for quantitative analysis. Had they taken account of the widely differing intensities with which the spectra of different metals appear under the same conditions, and had they chosen for comparison lines those homologous according to the more modern developments of spectroscopy, more might have been accomplished.

After preliminary work had shown that the gas used as ambient atmosphere was of little consequence, hydrogen was chosen for the work on account of the simplicity of its spectra. The greater part of the work was done in hydrogen at atmospheric pressure, the remainder in the open air. The spark tube was of the special design described in a previous paper.[2] The spark was excited by a small 10,000-volt transformer fed at 100 to 400 watts by means of a control rheostat in the primary. Spectra were recorded photographically by

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[1] *Proc. R. S.*, **21**, 507–508, 1873.

[2] P. G. Nutting, "Metal-Gas Spectra," *Bureau of Standards Bulletin* No. 3.

means of a large model Fuess spectrograph. With each alloy, five spectra were taken side by side on the same plate. These five spectra were taken (1) with large capacity; (2) with large capacity and inductance; (3) with small capacity and series spark; (4) with series spark and large inductance added; (5) with neither capacity nor inductance.

The alloys were usually fused in graphite crucibles and cast in a mold in cylindrical rods 2 mm in diameter. Among the sixty combinations tried, fully a third proved to be eutechtic; on solidifying the fused mixture separated into parts having different composition. Cadmium and lead fuse up nicely with aluminium, but on cooling a button of pure, or nearly pure, aluminium freezes out on top. Tin and zinc, however, appear to form true alloys in any proportion with aluminium. Some of the magnesium alloys could not be cast and were glass-hard, but chips were ground into shape for use as electrodes on an emery wheel. Except in the case of lead, the tellurium alloys appeared to go over chemically into nearly infusible masses. The antimony-zinc combination is eutechtic, but as the eutechtic alloy mixes with either of the two metals in all proportions, it may, for spectroscopic purposes, be treated as homogeneous.

The problem of preponderance in the spectra of alloys clearly resolves itself into one of the vaporization of the electrodes, and of the excitation of this vapor to luminosity. Are the metals composing an alloy used as electrode vaporized by the passage of the spark in the proportion in which they are present? Once vaporized, does the mixed vapor obey the spectroscopic laws of mixed gases?

EXPERIMENTAL RESULTS

1. *Effect of presence of second metal.*—Considering the spectra as a whole, the spectra of an alloy are found to be entirely independent of each other. There are a few cases where the presence of a second metal appears to affect slightly the relative intensity of some of the lines in the spectra of the first, but the effects are insignificant. Four series of tests were made. In the first series the metals were mixed in equal parts; in the second, lead and zinc were mixed in varying proportion ranging from pure lead to pure zinc. In the third series, carried out really to test the identity of the so-called "long" and

"short" lines with those variant or invariant with inductance, two different pure metals were used as electrodes. Finally the spectra of five of the more easily vaporizable metals were re-examined[1] in Plücker tubes.

When alloys were made up of equal parts by volume of two metals, in every case the metals appeared to be vaporized in the proportion in which they were present; that is, there was no selective vaporization (fractional distillation) such as might occur on heating to a high temperature. In other words, the vaporization produced by a spark is a *skin* effect, taking place just at the bounding surface between electrode and gas. This might be expected from the fact that the electrode fall of potential is chiefly confined to the region immediately adjacent to the electrodes, and the product of fall of potential and current is a measure of the energy loss. The results obtained are tabulated below. A plus sign indicates that spectra of the alloy were photographed; a minus sign indicates lack of success in preparing the alloy, or that the alloy proved to be eutechtic, while blank spaces are left where no trials were made.

	Zn	*Sn*	*Sb*	*Pb*	*Mg*	*Hg*	*Cd*	*Bi*
Al........	+	+	−	−	+	−	−	+
Bi.............	+	+	+	+			+	
Cd.............	+	+	+	+		+		
Hg.......... .	+	−	−	+				
Mg............	+	−						
Pb.............	+	+	+					
Sb.............	+	+						
Sn.............	+							

Tests were also made of the alloys copper-zinc, copper-cadmium, arsenic-lead, tellurium-lead, thallium-lead and thallium-zinc.

There was also considered the possibility that, if one of the metals of an alloy electrode oxidized more readily than the other, a fleck of the oxide of one metal might form on the surface and the current pass out chiefly through this fleck on account of the low electrode fall from oxides. Such an effect would alter the relative intensities of the two metallic spectra. Some metals, notably lead and tellurium, form black hydrides in great quantities when the spark passes in hydrogen, but in no case was the effect mentioned observed. Some

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[1] "The Spectra of Mixed Gases," *Astrophysical Journal*, **19**, 105–110, 1904.

of the lead and zinc alloys were used in oxygen and air as well as hydrogen, but without observable effect on spectral preponderance.

The nine lead-zinc alloys, mixed in percentages of 5, 10, 20, and 50 per cent. of each metal, showed the same independence of the two spectra. From these spectrograms the proportion in which the metals were present might be estimated with fair accuracy by comparing homologous lines, but there would always be a tendency to overestimate a small proportion; 5 per cent. lead in zinc (or zinc in lead) appears more like 10 per cent. As an extreme case, some impure tellurium, estimated to contain 15 per cent. lead, was found, on analysis, to contain but 3 per cent. In agreement with Lockyer, the variant ("short line") group of lead lines $\lambda\lambda$3828, 3833, 3842, 3854, do not appear until the proportion of lead reaches about 30 per cent., while all the six prominent invariant ("long") lines appear prominently when but 5 per cent. lead is present. The relative natural intensities of spectra to be taken into account in making estimates of relative intensity is given in the accompanying table of data taken from an earlier paper.[1] The results all refer to the spark in hydrogen. The fourth column refers to the spark with large capacity; the fifth, to the spark with large capacity and large inductance added; the sixth, designated "arc," refers to data taken with neither capacity nor inductance added. The following column, marked "per cent. invariant," indicates roughly the proportion of the lines of the metallic spectra which are not cut out by adding inductance. The last column, headed "hardness," refers to the brightness with which the metallic spectrum comes out in hydrogen. Calling silver and platinum 1 and lead 10, intermediate estimates for the other metals were made from the photograph by two observers at different times. Estimates of "hardness" rarely differed by more than unity. In air the spectra of aluminium, antimony, tin, and tellurium are much "softer" than in hydrogen, while cobalt, chromium, iron, and nickel are much "harder," the spectra of the remaining metals being but little affected in hardness by change of atmosphere.

The use of two different pure metals as electrodes affords a convenient and effective means of classifying lines according to variability. The "short" lines of Lockyer are the lines cut out by

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[1] "Metal-Gas Spectra," *Bureau of Standards Bulletin* No. 3.

	Atomic Weight	Temp. of Fusion	No. Lines E. and H.	Intensity in *H*			Per cent. Invariant	Hardness
				Cap.	Ind.	Arc		
Al	27	657°	105	30	30	10	40	5
Bi	208	268	98	90	90	80	60	8
Cd	112	321	113	40	40	30	90	7
Cu	63	1084	260	10	10	10	95	2
Hg	200	−39	94	70	90	70	95	8
Mg	24	750	46	60	60	40	95	6
Pb	205	326	74	80	90	30	90	10
Sb	119	632	160	50	40	20	80	7
Sn	118	232	87	60	50	30	70	8
Te	127	525	79	80	10 ?	10 ?	20	5
Tl	204	290	16	40	60	30	80	8
Zn	65	419	108	30	30	10	95	5

inserting inductance or removing capacity, and these same lines show more or less stubby in the spectrograms, according to variability. On the other hand, the "long" or "arc" lines which are invariant, extend entirely across the spark image. Adding capacity lengthens all lines, while adding inductance shortens them. There exist all intermediate degrees of variability, so that, instead of classifying lines as either variant or invariant, it would be preferable to grade them on a scale of ten or one hundred. And by this method of throwing a real image on the spark longitudinally upon the spectrograph slit, even the faint lines of a "soft" spectrum like that of silver may be easily graded. It is of interest here to note that, while the long invariant lines show in the spectrum of an impurity only 2 per cent. of which is present in an alloy, the variant lines may not show until the proportion reaches 30 per cent.

Finally the results for the spark spectra of alloys were connected with the work on mixed gases in Plücker tubes by vaporizing zinc, cadmium, mercury, tellurium, thallium, and arsenic, with hydrogen and with each other, in Plücker tubes. Even in this case the invariant "arc" lines are the first to appear, whether in the presence of hydrogen or the vapor of another metal. And inductance appears to cut out the same lines from a Plücker tube spectrum that it cuts out from the spark spectrum of the same metal. However, the only lines studied were half a dozen belonging to cadmium, thallium, and tellurium, there being no other variant lines available in the Plücker tube spectra of metals.

2. *Effect of varying electrical conditions.*—A 10,000-volt spark, with and without capacity, inductance and series spark, and a 120-volt arc were used in these tests. The first series of photographs were taken with the schedule given on page 132; a later series was taken with the schedule: (1) spark with large capacity; (2) spark with capacity and inductance; (3) spark with neither capacity nor inductance, i. e., a 10,000-volt, 10-milliampere alternating arc; (4) 120-volt, 4-ampere direct current arc; (5) same with but one ampere current.

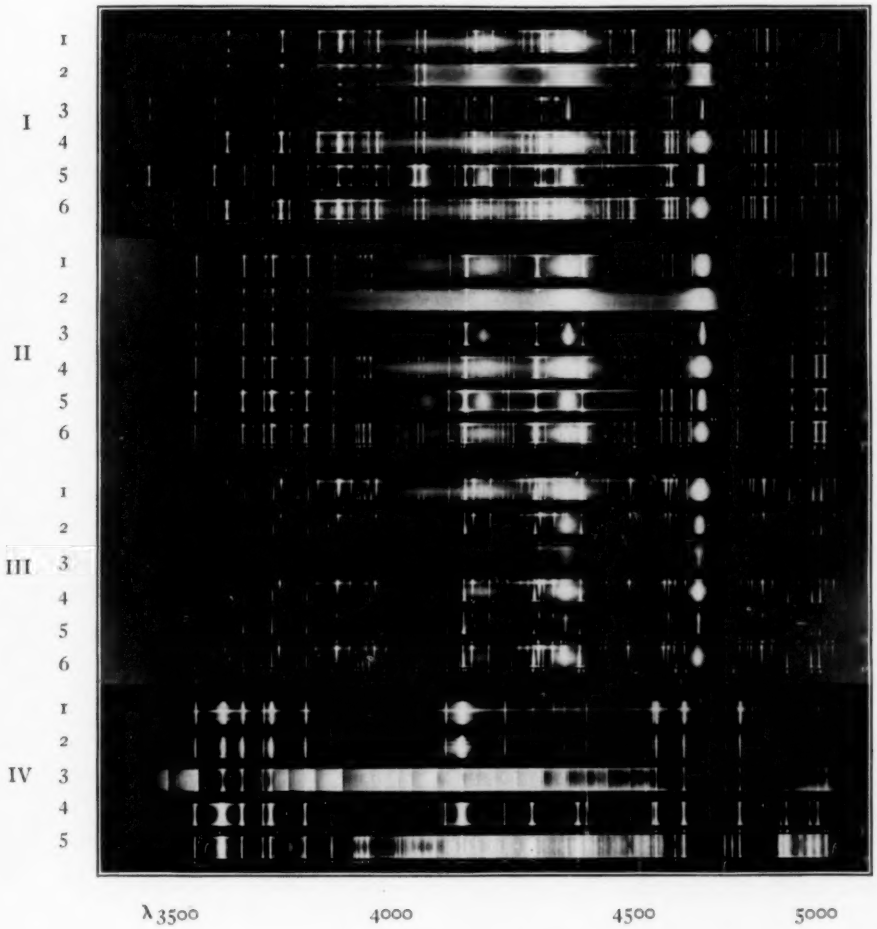
While these changes in the excitation produce enormous changes in the relative intensities of individual lines, the spectra of the different metals in an alloy are affected independently. If the manner of excitation does affect preponderance, it does so but slightly, and the effect is masked by the changes in individual lines.

3. *Effect of atmosphere.*—Tests made in atmospheres of hydrogen, oxygen, air, and in arsenic and mercury vapor, showed no effect of change of atmosphere, so far as the "long" invariant lines were concerned, and only a very few cases where even the variant lines were affected. For example, the relative intensity of the variant lead group near λ 3800 is different in air and arsenic from what it is in hydrogen and oxygen, but the smilar variant groups in the aluminium spectrum near λλ 3600 and 4500 remain unaffected even in an atmosphere of mercury vapor.

4. *Effect of atomic weight.*—From results obtained with other gases in Plücker tubes, we should expect, other things being equal, and the metals in the electrodes being vaporized in the proportion in which they are present, that the spectrum of the metal of greater atomic weight would predominate in the spectra of the low potential arc and the spark with inductance, while in the spark with capacity, the spectra of the two component metals (allowance being made for "natural intensity" and "hardness," see page 134 above) would be of equal prominence. This appears to be the case. The heavier metals, lead, mercury, thallium, and bismuth, strongly predominate in arc and inductance spectra over the lighter, magnesium, aluminium, copper, and zinc; while cadmium, tin, antimony, and tellurium predominate or are subordinate according to the weight of the metal with which they are associated.

But in the spectra of the spark with capacity and the high poten-

PLATE IV



Spectra of Alloys

I. Aluminium-bismuth alloy; 1, spark with small capacity; 2, spark with neither capacity nor inductance; 3, series spark with inductance; 4, series spark without inductance; 5, large capacity with inductance; 6, large capacity without inductance. II. Zinc-lead alloy, 5 per cent. zinc, 95 per cent. lead, same schedule as I. III. Spark with one electrode lead and the other bismuth, showing relation of "short" lines to inductance effect. Same schedule as I and II. IV. Arc and spark spectra, lead-cadmium alloy; 1, 120-volt, 4 ampere arc; 2, 120-volt, half ampere arc; 3, 10,000-volt, 0.04 ampere spark with neither capacity nor inductance; 4, spark with large capacity and inductance; 5, spark with large capacity and without inductance.

tial, low-current arc, the effects are more complex. Zinc spectra, for example, are relatively brighter, while antimony spectra are fainter, than we should expect. I have been unable to formulate any general conclusions to cover this case. The lack of co-ordination in the results may be due to the presence of the third gas in large proportion, the steep wave front of the electrical discharge, an alteration in the corrections for "natural intensity," and spectral "hardness," or perhaps to a slight tendency toward fractional vaporization. Like the three-body problem, the three-gas problem appears to admit of no simple solution.

Summary of conclusions.—From the results of this work it would appear that:

1. In the arc and spark spectra of alloys the spectra of the component metals are independent of one another. Intensities present in any proportion do not affect the intensity of a spectrum as a whole, except to decrease its intensity.

2. Varying electrical conditions of excitation, or varying the ambient atmosphere, does not affect the relative intensity of the component spectra.

3. In the arc and spark with inductance, other things being equal, the spectrum of the component of greater atomic weight will be brighter.

4. Spectroscopic quantitative analysis, to within an error of perhaps 5 per cent., appears to be practicable, provided: (*a*) selected lines of similar character are used for comparison; (*b*) spectra are taken with an arc or spark with capacity and inductance, with sufficient current to produce plenty of metallic vapor in proportion to the ambient gas; (*c*) allowance is made for differences in the natural intensity and hardness of the spectra of various pure metals taken under the same conditions; (*d*) allowance is made for differences in the atomic weights of components, provided these differ by a considerable amount.

In practice, (*a*) the presence of impurities in electrodes is of little consequence; (*b*) when alloys are used as electrodes, it is useless to attempt to intensify the spectrum of either component by varying the conditions under which the arc or spark is produced.

BUREAU OF STANDARDS,
Washington, D. C.,
July 1905.

CALIBRATION OF A WEDGE PHOTOMETER

By JAMES D. MADDRILL

In the spring of 1900 the Lick Observatory agreed to take part in a determination of standards for faint stellar magnitudes, in accordance with a co-operative plan suggested by Professor E. C. Pickering, director of the Harvard College Observatory. Wedge photometers of a special type (see description by Mr. J. A. Parkhurst, *Astrophysical Journal,* **13,** 249), devised by Professor Pickering and provided for from the Rumford Fund of the American Academy, were distributed from the Harvard College Observatory. Photometer No. 3, containing photographic Wedge III, was received at the Lick Observatory in May 1900. The part assumed by the Lick Observatory involved the comparison of stars near the limit of the great refractor with stars of about the twelfth magnitude. With the apparatus as originally arranged, it was found that the artificial star could not be made bright enough for comparison with stars brighter than about the fourteenth magnitude in the great telescope. The small incandescent lamp originally inserted at *S* through the side of the tube (see Fig. 1) was therefore removed, and a brighter lamp *A* was placed lengthwise at the end of the tube. At the same time, the piece of blue glass supplied, was replaced by a piece *B* of darker blue. The resulting artificial star is well defined and is perhaps a trifle more reddish than the average real star. In addition, the cell carrying the two shade glasses *L* was attached to the tube *T* so that either or both could be interposed in the rays of the real star. After the drawing of Fig. 1, a slight alteration was made by which the lamp *A* can be moved an inch closer to the diaphragm if desired. The brightness is thus increased by about a magnitude and a half. The image in any possible position of the lamp can be made nearly as small and sharp as the image of the real star under the best conditions.

Owing to the pressure of work in other directions, the program was not entered upon here until June 1902. The observational part was then taken up by Dr. Aitken, assisted by several others, and completed in January 1904. The photometer has been used to some

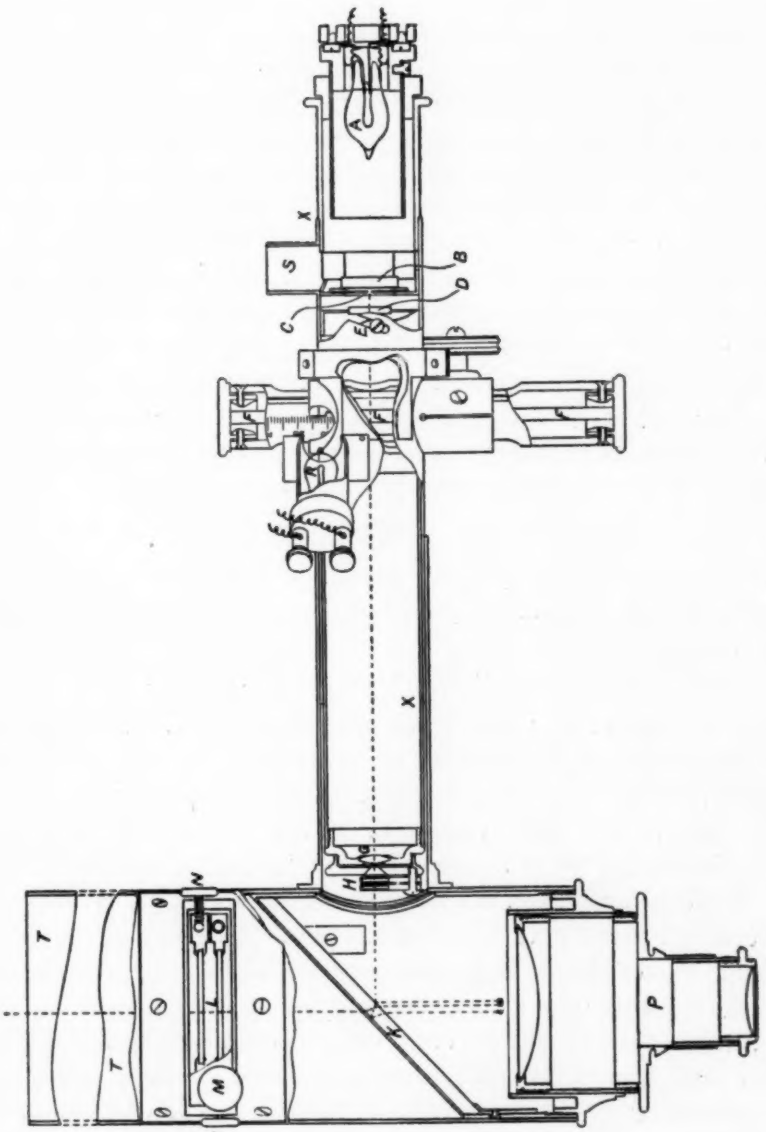


FIG. 1

extent, besides, in the observation of variable stars. The unsatisfactory condition of our knowledge of the constants of the instrument has delayed the final reduction of the measures.

Preliminary star measures of the "absorption" (strictly absorption and reflection) of the wedge, made by Dr. Aitken, early showed departures from the theoretical linear relation between scale reading and absorption in magnitudes. Rather smaller values of the relative absorption per division were found at large than at small scale readings. It became evident that for accurate determinations an "absorption" *curve* of the wedge would be required. Other observers found similar difficulties with their wedges. Mathematical and empirical absorption curves were derived. Two, as yet unpublished, have come to my notice. These were for wedges cut from the same plate as the Lick wedge; similar results might therefore be expected for the Lick wedge. In a preliminary investigation of the Virginia wedge, Professor Stone found the mathematical relation

$$\sigma = 0.132 - 0.00080\,(m - 31.8) \ .$$

This was obtained by varying the aperture of the object-glass in known ratios. The integral curve, passed through the origin, is, using our own notation,

$$m = (0.1574 - 0.00040d)\,d \ .$$

The empirical curve "A 12," determined by Mr. J. A. Parkhurst for Wedge V at the Yerkes Observatory, is shown in Fig. 2 (*A*). I believe this was obtained by means of a wheel or polarizing photometer, point images being compared. A few preliminary measures of *Pleiades* stars by Dr. Aitken were used here in the attempt to derive a curve, assuming the values of the absorption per division to be constant over stretches of five divisions. A curve following the general direction of Parkhurst's resulted. These preliminary measures showed that a very great number of star measures would be required to determine the curve with sufficient accuracy. On the other hand, it seemed that laboratory comparisons, made as nearly as possible under the same conditions as those of practice, should yield the curve directly, and with greater facility. Accordingly, at the suggestion of Director Campbell, the photometer was taken to Berkeley in June 1904 and adapted for comparison with the Lummer-

Brodhun photometer, kindly placed at our disposal by Professor Slate.

For convenience in making settings of the laboratory photometer, the tube *X* (see Fig. 1), carrying the wedge tube *FF* and terminating at *X* and *X*, was mounted in the optical axis on a carriage composed of the prism-box and one of the lamp-holders. This carriage was movable as a single rigid system; the lamp at the left end of the bench, together with its blue glass, diffusing surface, and diaphragm, being kept fixed. One of the moving pointers was read at a given setting, and the distance, d_2, of the screen from the fixed source, s_2, was obtained by subtracting a readily determined constant. Settings were made with the wedge at 0, 5, 10, ... , 65. The measures were then repeated in the reverse order, from 65 to 0. The following are illustrative settings, made 1904, June 13:

	Wedge				
	0	5	10	15	
	cm	cm	cm	cm	
Settings.......	143.1 142.4 143.0 142.2	145.5 144.3 143.8 144.9 144.2	158.3 160.9 159.6 159.3	187.1 186.9 184.8 186.2	Diaph. I (2.92 cm) over s_2. Diaph. 0.29 cm over wedge. Reading of pointer, with meter-stick touching s_2 and screen 163.66 cm.
	142.68	144.54	159.52	186.25	

A small correction, due to decreasing effective illuminating surface with increasing distance of the screen from the source, was taken into account by measuring distances, d_2, from ***diaphragm*** to screen. The distance from s_2 to the diaphragm was 1.32 cm. d_2 was therefore 64.98 cm less than the reading of the pointer. The reduction to magnitudes was carried on as follows ($\Delta m = \Delta \frac{\log(d_2^2)}{\cdot 4} = 5\Delta \log d_2$) :

Wedge	d_2	$\log d_2$	m
	cm		
0............	77.7	1.8904	0.000
5............	79.6	1.9009	0.052
10............	94.5	1.9754	0.425
15............	121.3	2.0839	0.968

When the length of the bench forbade further settings with a given diaphragm over s_2, a smaller diaphragm was used and settings were continued, a connected series being obtained by beginning at the last position of the wedge. The two following series resulted:

Wedge	I	II	Diff.	Mean
d	m	m	m	m
0	0.000	0.000	0.000	0.000
5	0.052	0.038	−0.014	0.045
10	0.425	0.402	−0.023	0.414
15	0.968	0.968	0.000	0.968
20	1.458	1.384	−0.074	1.421
25	1.809	1.736	−0.073	1.772
30	2.157	2.058	−0.099	2.108
35	2.601	2.512	−0.089,	2.556
40	3.059	2.944	−0.115	3.002
45	3.397	3.342	−0.055	3.370
50	3.602	3.552	−0.050	3.577
55	3.655	3.566	−0.089	3.610
60	2.562	.....	.....	2.5
65	0.108	.....	.....	0.10

The difference at 15 is probably due to a particle of dust or an over-setting of the wedge in the second series. At this point the mean curve is somewhat higher, relatively, than the other curves for this wedge or the other wedges. The differences could be smoothed and perhaps ought to be, as irregularities of the sort suspected at 15 might easily occur. It has not been done here, but can be done at any time if desirable. The mean curve is plotted in Fig. 2 (*D*), and designated as the Laboratory Curve.

The marked similarity of the laboratory curve to Parkhurst's Curve A 12 for Wedge V was noticed as soon as it was plotted. Their points of inflection occur at the same scale readings; they might be brought nearly into coincidence by a simple change of slope—except near the dense end, where the wedges are certainly different. The star observations by Dr. Aitken required an average slope ("wedge-constant") about equal to that of Parkhurst's Curve. It was fully expected that the laboratory investigation would yield such a slope, special care having been taken to place the light-diffusing surface at a distance from the wedge greater than 5 cm—in accordance with the conclusions reached by Mr. E. S. King on the effect of distance of the source of light from the photographic wedge.[1] It was necessary, however, because the Lummer-Brodhun photometer compares areas, and because the cone of rays forming the artificial star is less than a millimeter in diameter at the wedge, to place a diaphragm near the wedge. Lenses might have been used to render the divergent pencils nearly parallel at the wedge; but, aside from the departure from the conditions of practice, loss of light would have resulted, and new internal reflections would have been introduced. Light

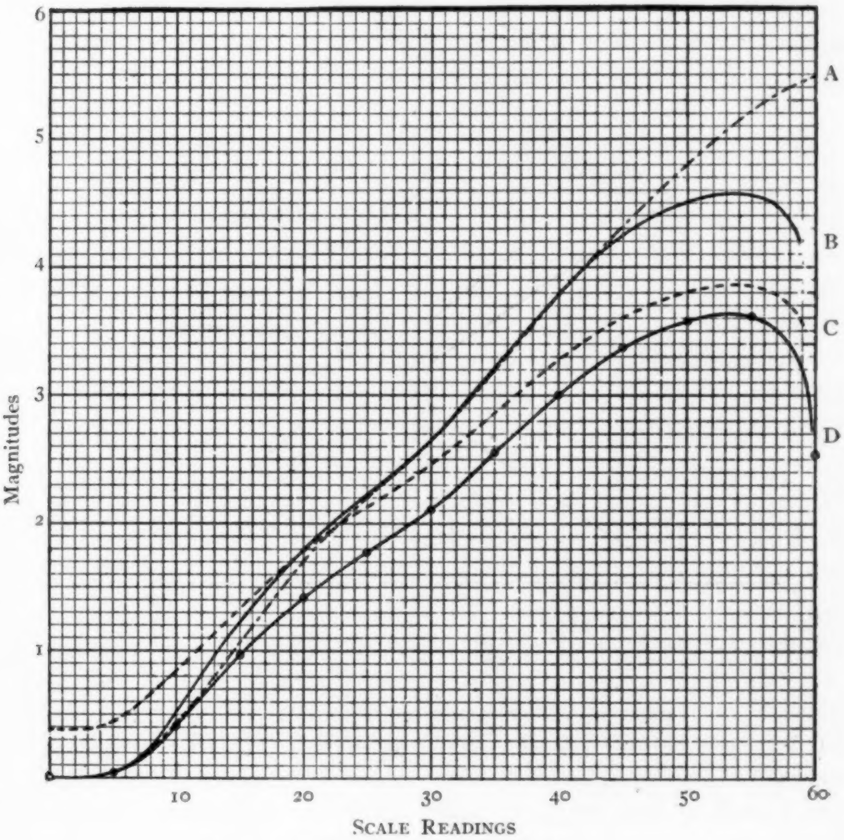


FIG. 2.—*A*=Curve A 12– V (Yerkes).
B=Wedge Curve– III (Lick).
C=Laboratory Curve– III (Harvard).
D=Laboratory Curve– III (Lick).

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[1] *Annals of Harvard College Observatory*, **41**, 244.

conditions required the use of a diaphragm considerably larger than would otherwise have been employed. Its aperture was 0.29 cm. The construction of the wedge tube made it convenient to place the diaphragm 1.2 cm from the wedge (film) and inconvenient to place it nearer. Just beyond the diaphragm, and separated from it by a cardboard washer, was the piece of blue glass ordinarily used with the artificial star. The distance from wedge to diffusing surface was 8 cm. The area of wedge uniformly illuminated (a_1, Fig. 3) was 0.26 cm in diameter, and the boundary of the area (a_2) surrounding, illuminated by smaller parts of the diffusing surface, was 0.94 cm in diameter. a_1 and a_2 correspond somewhat to umbra and penumbra, with light substituted for shadow.

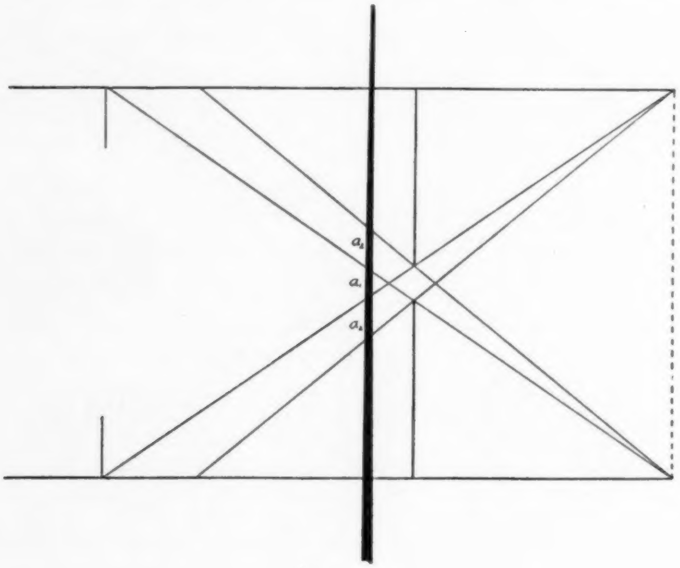

FIG. 3

It has occurred to me that the following might be an explanation of the phenomena observed by King and myself. The diffusion of light by the silver grains is greater at the dense end of the wedge than at the thin end. Little of the light transmitted at a_2 without diffusion reaches the screen or field of the reference photometer, because of the greater angle of these rays with the optical axis. Much of this light is saved by diffusion at the dense end of the wedge. There will be some loss of the light of a_1 by diffusion at the same time, but this will probably be less than the gain by a_2, because of the smaller angle of the rays of a_1 with the optical axis. Thus arrangements in which a_2 bears a large ratio to a_1 will yield smaller values of the slope of the curve than arrangements in which the ratio is smaller. Larger and practically identical values result from star determinations or comparisons with photometers employing point images, a_2 being very small.

After the measures of the wedge, determinations of the absorption of the shade-glasses were made with the Lummer-Brodhun photometer. The small shade (No. 2) next the projecting lens of the artificial star was mounted (with the projecting lens) in the optical axis, 5 cm from the blue glass that was placed against the rice-paper diffusing surface. Twelve settings, in the order: 3 shade out, 6 shade in, 3 shade out, gave 1.72 ± 0.001 magnitudes absorption.

The shade-glasses *L* were then adapted for determination. A diaphragm of aperture, 0.6 cm, approximately equal to the diameter at the shades of the cone of rays from the real star in the 36-inch or 12-inch refractors, was placed between the shades at the position of the cone in practice. Shade I was 8 cm, and Shade II 9 cm, from the source, which was left as in the measures of the small shade No. 2. Twenty settings, in symmetrical order, gave 0.89, 0.86, and 1.75 magnitudes, respectively, for the absorption of Shade I, Shade II, and both shades together.

A second laboratory investigation of the wedge was made in January 1905, with a view to determining the effect of position of diffusing surfaces, diaphragms, color, etc. Special attention was paid to securing greater intensity of illumination. The 32 candle-power incandescent lamps of June were replaced by 100 candle-power incandescent lamps, the lamps being connected always in parallel on the same circuit. A slit aperture, 0.165 cm by 1.2 cm, placed close to the glass of the wedge, was turned at right angles to the direction of motion of the wedge. The area a_1 was about 0.06 by 1.2 cm, and the area a_2 about 0.3 by 1.3 cm. Thin rice-paper, 4 cm from the lamp, and 8 cm from the wedge, diffused the light transmitted. Ten determinations of the relative "absorption" between scale readings 10 and 50, with a piece of rice-paper touching the slit, gave 3.19

magnitudes, with a mean range of ±0.03. These observations were made with a variety of arrangements of diaphragms, with the candle-power reduced (by resistance in the circuit) to 70 and to 1, with blue glass, with red glass, etc. The sources were made similar in each comparison. No effect of color or candle-power was detected. The numerical result is practically that obtained in June (1904), 3.16 magnitudes, and seems to indicate that the blue glass placed 1.4 cm from the wedge in June behaved much as a diffusing surface. Eight determinations with rice-paper 3.0 cm from the wedge gave 3.34 magnitudes. Three, with rice-paper at both positions, gave 3.10 magnitudes. With no diffusing surface between the wedge and the rice-paper 8 cm away, three determinations gave 3.73 magnitudes. No arrangement tried would yield a result nearer 4.0 magnitudes, given by the star measures. In nineteen of the observations, settings were made at scale reading 30 as well as at 10 and 50. The mean of the nineteen ratios of the relative absorption between 10 and 30 to that between 10 and 50, was 0.548, mean residual ±0.006, probable error ±0.0013. The uniform value of this ratio shows that the different curves resulting from the various arrangements of the laboratory apparatus, could all be obtained by stretching one of them, say the laboratory curve of June 1904. It therefore seemed reasonable that a curve accurate enough for all practical purposes would be obtained by stretching the laboratory curve in a ratio to be determined from star measures.

A series of measures of *Pleiades* stars, selected from the list determined by Müller and Kempf,[1] was then made for this purpose by Dr. Aitken. Fifty-one pairs of stars were observed, the average range of a pair being 1.9 magnitude. The 12-inch refractor was employed, part of the time with full aperture and part of the time with 6-inch and 3-inch apertures. The shade glasses were used in some of the measures; the values of their absorption were taken from the laboratory determination. The stretching factor was derived from 51 equations of the form $f=\frac{\Delta M-\Delta m}{\Delta m}$, M being the magnitude from Müller and Kempf, and m the magnitude from the laboratory curve. The weight assigned to each determination was Δm. A

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[1] *Astronomische Nachrichten*, **150**, 203, 1899.

fact already noted by Dr. Aitken, that the smaller apertures or use of shades give greater absorption per scale division, appears in the following tabulated results:

Aperture	Shades	Pairs	$[\Delta m]$	$[\Delta M-\Delta m]$	f	p. e.
12 in..........	none	15	16.23	+3.99	+0.246	±0.015
6	none	16	24.18	+6.35	+0.263	±0.020
6	both	6	12.30	+3.58	+0.291	±0.031
3	none	14	22.45	+7.37	+0.328	±0.049

It was thought best to combine these determinations in one result to be used for all apertures and conditions. In practice, a large range of wedge will rarely be used in comparisons, and single determinations will be liable to actual errors much larger than inaccuracies thus introduced. The tabulated determinations were therefore weighted in inverse ratio to the squares of their probable errors and combined, with the final results, $f = +0.261 \pm 0.011$. The ordinates of the laboratory curve were then increased by 0.261 of their own length. The resulting curve is plotted in Fig. 2 (*B*), and designated as the Wedge Curve. For practical use, the thousandths have been dropped, and the values tabulated as *m* with the argument scale division, *d:*

TABLE OF *m* WITH ARGUMENT *d*

d	00	10	20	30	40	50
0	0.00	0.52	1.79	2.66	3.79	4.51
1	0.00	0.67	1.89	2.76	3.90	4.54
2	0.00	0.82	1.98	2.87	4.00	4.56
3	0.01	0.96	2.07	2.99	4.09	4.57
4	0.02	1.10	2.15	3.10	4.17	4.57
5	0.05	1.22	2.23	3.22	4.25	4.56
6	0.10	1.34	2.31	3.33	4.32	*
7	0.17	1.46	2.39	3.45	4.38	*
8	0.26	1.57	2.47	3.57	4.43	*
9	0.38	1.68	2.56	3.68	4.47	*
10	0.52	1.79	2.66	3.79	4.51	*

PP	11	12	13	14	15
1	1.1	1.2	1.3	1.4	1.5
2	2.2	2.4	2.6	2.8	3.0
3	3.3	3.6	3.9	4.2	4.5
4	4.4	4.8	5.2	5.6	6.0
5	5.5	6.0	6.5	7.0	7.5
6	6.6	7.2	7.8	8.4	9.0
7	7.7	8.4	9.1	9.8	10.5
8	8.8	9.6	10.4	11.2	12.0
9	9.9	10.8	11.7	12.6	13.5

The following will illustrate the method of reduction that I have been using. The stars here measured are among the faintest on the Rumford program.

FIELD OF *R DRACONIS*-STANDARDS FOR FAINT STELLAR MAGNITUDES

Wed., 1903, Aug. 19, 9:50–10:56, P. S. T. Obsr., R. G. Aitken.

*	*d*	*m*	Shades	Obs.	*M*	*k*	Obs. Mag.	MEAN RANGE	
								d	*m*
a I II.....	10.60	0.61	1.75	*k*− 1.14	=11.12	12.26	11.51	±1.2	±0.18
b I II.....	12.78	0.93	1.75	− 0.82	11.82	12.64	11.83	0.9	.13
c I II.....	14.51	1.16	1.75	− 0.59	12.20	12.79	12.06	1.2	.14
d I II.....	18.66	1.64	1.75	− 0.11	12.38	12.49	12.54	0.7	.08
e I II.....	16.16	1.36	1.75	− 0.39	12.67	13.06	12.26	0.6	.07
								Mean	.12
q.........	38.87	3.67	....	+3.67	.....	.....	16.32	1.6	.18
r.........	40.37	3.83	.....	+3.83	.....	.....	16.48	1.4	.15
s.........	43.13	4.10	.....	+4.10	.....	.....	16.75	1.7	.14
t.........	44.14	4.18	....	+4.18	.....	.....	16.83	0.8	.06
u.........	47.99	4.43	....	+4.43	.....	.....	17.08	2.2	.10
					Mean	12.65		Mean	.13

The first three columns are taken from the record book and table. Column 4 gives the value of the shades employed (indicated in column 1 by Roman numerals). The measures being purely differential, a constant *k* is introduced in column 5 to express the absolute magnitude. The reference magnitudes *M*, in column 6, are preliminary values supplied by the Harvard College Observatory. The individual determinations of *k* from the observation equations of columns 5 and 6 are entered in column 7. The mean value, at the foot of the column, is applied successively in column 5, and the result of the observation is given in column 8. It will be noticed that the scheme at the same time conveniently exhibits data for an inter-adjustment of the reference magnitudes *M*. The corresponding data from all the observations can finally be combined and the best relative system of magnitudes *M* obtained. For example, observations of this field by the same observer on two other nights—a different combination of shades being employed each night—give:

*	II	III	Mean I, II, III
a	11.52	11.45	11.49
b	11.76	11.87	11.82
c	12.29	12.17	12.17
d	12.29	12.31	12.38
e	12.31	12.40	12.32
q	16.53	16.41	16.42
r	16.71	16.45	16.55
s	16.69	16.67	16.70
t	16.77	16.68	16.76
u	17.00	17.01	17.03

If the reference magnitudes given by Harvard College Observatory were determined from three nights' work with a photographic wedge photometer, the magnitudes to be adopted finally should probably be the means of the values found at Harvard, Yerkes, and Lick observatories. The individual and final results for ***q, r, s, t, u,*** are unaffected by such adjustment made from any number of ***complete*** observations. Columns 9 and 10 may be added to give an idea of the accuracy of the results.

Since the determination of the curve adopted, I have plotted the curve of the wedge from the means of the measures of its absolute absorption (and reflection) made at Harvard by Messrs. King, Bard and Cram (*Annals of Harvard College Observatory*, **41,** 239, Wedge III). A plot of these measures shows that the maximum lies at about 3.2 of the scale there used. The maximum of the Harvard curve was then placed opposite the maximum of the Laboratory curve in Fig. 2. If the ordinates of the Harvard curve be diminished by 0.39 magnitude so that it will pass through the origin, the differences L.O. – H.C.O. become:

d	*m*	*d*	*m*	*d*	*m*
0	0.00	20	+0.03	45	+0.17
5	0.00	25	+0.03	50	+0.18
10	−0.04	30	+0.04	55	+0.14
15	+0.03	35	+0.09	59	+0.02
		40	+0.12		

These would be reduced materially by applying a stretching factor of about +0.05.

LICK OBSERVATORY,
June 1905.

MINOR CONTRIBUTIONS AND NOTES

ON SPECTROSCOPIC OBSERVATIONS OF THE ROTATION OF THE SUN

The author of the review[1] of my paper has criticised the method by which the slit in my apparatus is illuminated, and he has expressed the doubt that my results on the rotation of the Sun may be seriously affected by what he considers an obvious deficiency introduced by the employment of the heliometer as the means for throwing the images of the opposite solar limbs upon the slit. He remarks that this defect in the apparatus will at once be recognized by anyone who has worked in stellar spectroscopy. I can assure him that it did not escape my attention before I began my observations, and that I did not enter upon a laborious and important research of this kind, which is indeed "exacting in its requirements," without having satisfied myself that the alleged deficiency does not exist in my measurements. Indeed, if the observations were vitiated by such errors as are pointed out by my critic, their value would, in my own opinion, be extremely doubtful. But his adverse conclusion has obviously been reached on an incomplete consideration of the apparatus and the method adopted in my observations. As the question is admittedly of general importance for solar research, and especially since the remarks against my investigation in the article referred to may be apt to misrepresent the character and quality of the work done at this observatory with regard to solar rotation, I may be allowed to correct the views of the reviewer by describing as clearly as possible the methods which I have employed in these delicate spectroscopic observations.

At the outset I remark that, apart from the design of the apparatus, the method is mainly the one originally adopted by Professor Dunér in his celebrated research. The spectra of two diametrically opposite points of the solar limb are thrown, side by side, into the focus of the viewing telescope, and in each of these two juxtaposed spectra the distances between two solar (iron) lines and two closely adjacent telluric lines are measured by means of a micrometer. If we denote the spectrum of the receding limb by r, that of the approaching limb by a, and if we call the measured distance between

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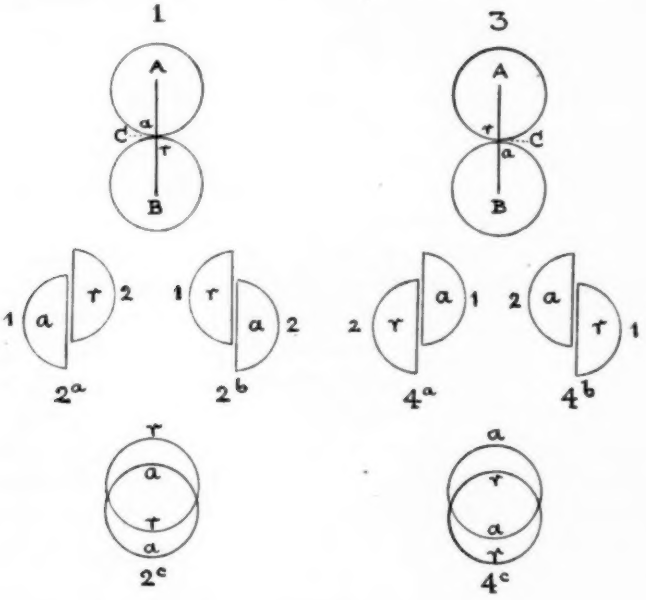
[1] *Astrophysical Journal*, **21,** 385, 1905.

the solar and telluric line in the first case D_r, and in the second case D_a, then $\frac{D_r - D_a}{2}$ represents the displacement due to rotation of a point in the observed heliographic latitude. Now, in order to bring the opposite points of the Sun upon neighboring parts of the slit, I employ a heliometer mounted in a horizontal meridianal position, which receives the light from a siderostat. The full aperture of the heliometer object-glass differs not much from that



of the collimating lens, so that if the centers of the two halves coincide, the cylinder of light leaving the collimator has nearly, although not quite, the diameter of the collimating lens. Obviously, by separating the optical centers by means of the heliometer screw, and by setting the heliometer at the proper position angle, we can bring any two opposite solar points upon neighboring points of the slit. Before the two images reach the slit, however, they pass through a large rectangular prism of excellent optical quality. By this contrivance we are enabled to rotate the images so that the line joining their centers falls exactly in the direction of the slit, which is constantly kept in the vertical direction. The arrangement of the images in the focal plane of the collimator is shown in Nos. 1 and 3 of the accompanying diagram, where AB denotes the position of the slit. Let us now suppose that in No. 1 the upper image is produced by the half object-glass

1 of the heliometer, and that this image has the approaching limb at *C*, while the lower image, with the receding limb at *C*, is produced by the half object-glass 2. With a sufficiently wide slit we see the pictures of these two halves upon the surface of the grating; and obviously in the position indicated by No. *2a*, the spectrum of the approaching limb is furnished by the semi-cylinder whose projection on the grating is denoted by the semi-circular surface on the left,[1] while the spectrum of the receding limb is furnished by the other half cylinder on the right. This is the picture which the author of the review had in his mind, and to which his criticism is fully applicable. Certainly the light of the two sections passes through different parts of the collimating lens, and also falls upon different parts of the grating, and hence the positions of the lines may be vitiated by the effects of bad focusing and of optical anomalies.

I admit that if I had confined my measurements to this one particular arrangement, my results would be open to grave doubt. But after this one observation had been made, I have invariably screwed the two half object-glasses of the heliometer into the other position. In this second observation, therefore, the former upper image has become the lower one, and *vice versa*. This does not apparently alter the arrangement in No. 1. We have still at *C* the approaching limb in the upper image, and the receding limb in the lower one. But the arrangement of the segments on the grating is now different, the light of the spectrum *r* being furnished by the left half, and that of the spectrum *a* by the right half. By combining the two observations we obtain evidently the same result as if the light of the spectrum *r* had been furnished by the full circle *rr* in No. *2c*, and that of the spectrum *a* by the circle *aa*.

But even now the set of measurements has not been considered complete. Another series of observations has been made in which the same solar points are thrown into the position shown in No. 3, where the upper image at *C* shows the receding point, and the lower image the approaching point of the Sun. This position can be attained in two ways, both of which have been employed, viz., by turning either the heliometer or the rectangular prism through an angle of 180°. The two corresponding arrangements of the segments on the grating are exhibited in Nos. 4*a* and 4*b*. Hence by again combining the two observations we arrive at the same result as if we had observed the receding limb by means of light coming from the full cylinder *rr*, and the approaching limb through light passing along the cylinder *aa*, as shown in No. 4*c*. Consequently, in the combined

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[1] The optical axis of the collimator is very nearly perpendicular to the surface of the grating.

four observations the light of both limbs has passed through identical parts of the whole apparatus, and therefore the objection raised in the review of my paper is untenable.

Far from being open to the criticism referred to, the method may even be said to possess certain optical advantages over the ordinary devices for projecting the limbs upon the slit. In as delicate an investigation as that before us it is essential that we should be able to ascertain the optical deficiencies of our instrument, and thus be in a position to eliminate, or at least to control, these during the progress of the work. It is obvious that the heliometric method, by supplying us with observations which, considered singly, are affected by errors of this kind, whereas these errors completely disappear from the general mean of a set of four judiciously arranged measurements, affords valuable tests of the optical quality of the spectroscope by which the results cannot but be benefited. The mechanical advantages of the heliometer are, I think, too obvious to require special mention. I trust the author of the review may now admit that his remarks that "grave doubt must necessarily be thrown upon some of the numerical results," and also that "it is difficult to see how the heliometer can be employed in a spectroscopic investigation so exacting in its requirements as that of the solar rotation," require essential modification.

On the other hand, I am greatly indebted to him for having pointed out the necessity of these spectroscopic observations of solar rotation, and I hope that his remarks on this point may impress upon solar physicists the importance of co-operative work in this direction.

J. HALM.

ROYAL OBSERVATORY,
Edinburgh, May 30, 1905.

PRELIMINARY NOTE ON "ORTHOCHROMATIC" PLATES

The usual spectrographic tests made for the purpose of determining the sensitiveness of "orthochromatic" plates are in general very much in error, owing to the means adopted for obtaining the negatives. The prismatic spectrum is altogether unsuited as a "standard": first, because of the selective absorption in the ultra-violet (and even in the visible violet) when prisms of dense flint, or direct vision spectroscopes, are used; while, second, the irrationality of the particular prism employed must be determined before any correct comparative estimate of the spectrograms can be made.

In the use of reflection gratings there is likewise an element of uncertainty on account of the selective absorption—no two gratings giving spectra directly comparable with one another in so far as luminosity is concerned—and it is this point which chiefly concerns the photographic plate.

The conditions governing a "standard" dispersion-piece are easily met by the adoption of a *transparent* diffraction grating manufactured as described by the writer,[1] which is obviously free from abnormality due to differences in the material of which it is formed, or to the process employed in its preparation.

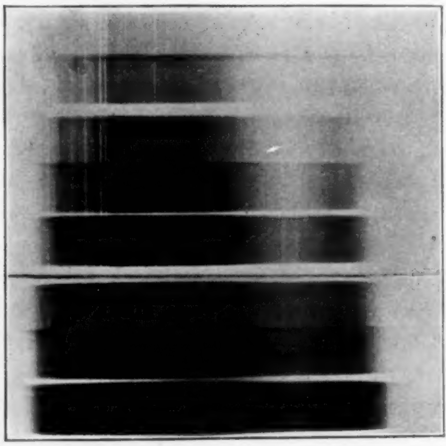

FIG. 1.—Sky Spectrum with increasing exposure on Seed "L. Ortho" plate.

Estimates based upon the negatives of the prismatic spectrum are very misleading on account of the crowding together of the colors in the red end and the elongation in the violet, thereby giving an apparently greater proportionate action in the least refrangible hues, for which the plate has been specially sensitized. Consequently there exists a considerable discrepancy in the curve of sensitiveness (even of the same plate) by different observers, no due allowance having been made for the increase in density towards the red, and decrease in the violet, consequent upon the prismatic irrationality already mentioned. It is, therefore, obvious that if the opacity of the red or yellow region be increased, while that in the violet be weakened, then due consideration must be given to this effect.

In the "sensitiveness-curves" appended to this paper, use was made of a transparent replica grating and the spectrum of the first order utilized. By means of a plate-holder furnished with rack and pinion consecutive exposures were made upon various plates with duration as follows: 2, 5, 15, 30 seconds, 1, 2, 4, 8, and 16 minutes.

In some cases a supplementary exposure of 25 to 30 minutes was given in order that the action of prolonged exposure might also be noted. A reproduction of one such plate is shown herewith.

In plotting the sensitiveness-curve a print was first made on Solio paper

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[1] See page 123.

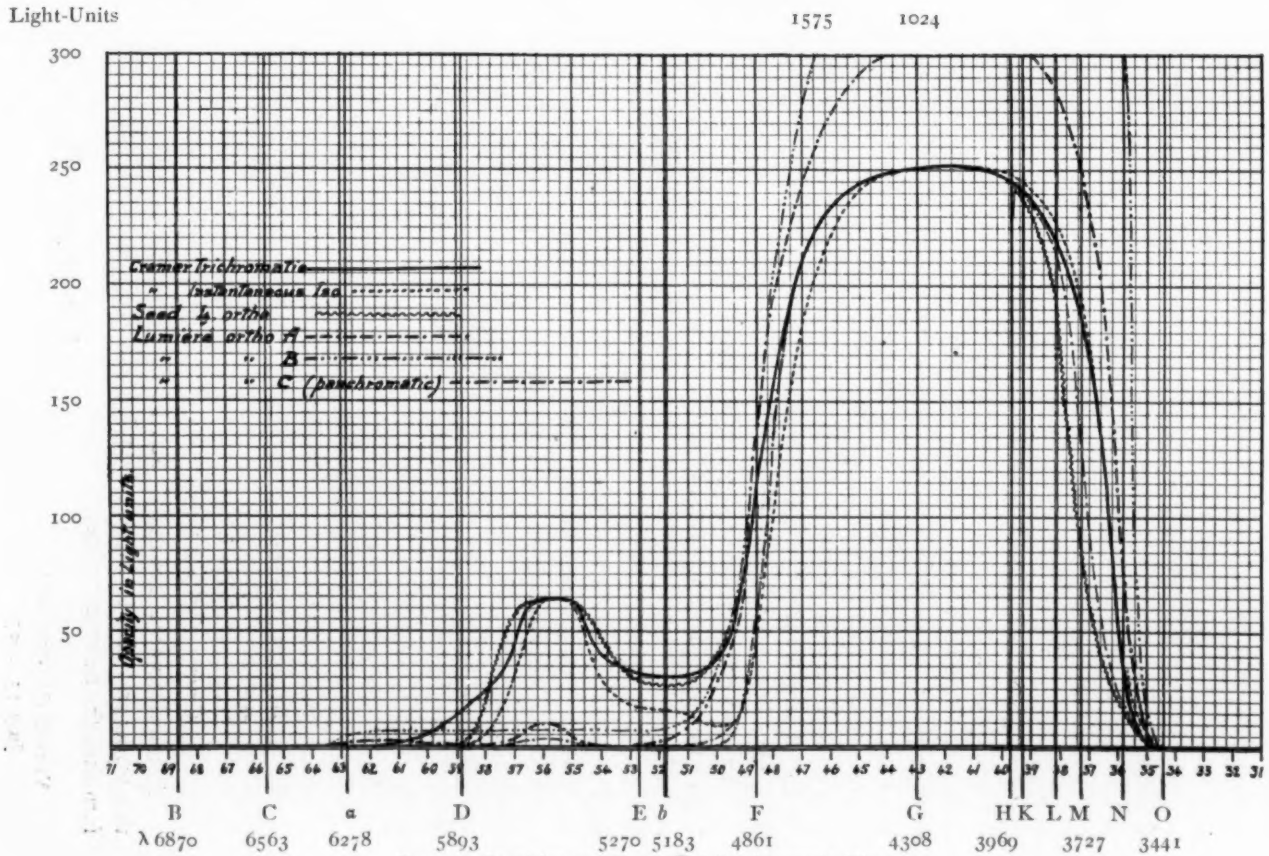

FIG. 2.—Sensitiveness-Curves of Isochromatic Plates.

from one of the plates, and that exposure selected which represented the highest allowable printing opacity—that is to say, that spectrum which was so opaque in the region of greatest sensitiveness that it was only with difficulty that the Fraunhofer lines would show on the print. This opacity was found to be represented by a light action of 256 units on a negative obtained from exposure behind a revolving Hurter and Driffield sector-disk, on the same plate, developed for the same length of time. On every other plate, therefore, that spectrum was selected for estimation whose greatest region of opacity corresponded to 256 light units on a similar plate exposed by disk. The curve was then plotted on squared paper, where the ordinates represent amount of light action (in units), and the abscissæ, wave-lengths; the variations in opacity throughout the length of the spectrum being estimated by small patches isolated by an opaque screen and in juxtaposition with the various opacities of the sector negative, the aperture ratio of which is accurately known.

It will be noted that two of the curves rise above the limit of the squared field, viz., Lumière "Ortho B" and "Panchromatic C." This was rendered necessary owing to the very slight action in the red end of the spectrum as compared with normal exposure in the blue-violet. The heights of the blue-violet curves in these cases were estimated simply by the ratio between that spectrum whose exposure time equaled normal exposure (=256 light units) and the time of the one selected for plotting, the opacity being much too intense for measurement.

It should be understood that these curves are representative only of "selective sensitiveness," no consideration whatever being given to the relative speed. Tests were made from the standpoint of the "Schwellenwerth" as being best suited to astronomical needs, where the principal point is to obtain developable action with the smallest light value. These test for the plates plotted may be tabulated in the following results:

Cramer "Instantaneous Isochromatic" = 1
Seed "Landscape Orthochromatic" = 3 per cent. less
Lumière "Ortho A" }
Lumière "Ortho C" } = 7 per cent. less
Lumière "Ortho B" = 14 per cent. less
Cramer "Trichromatic" = 40 per cent. less

Acknowledgments are due to Mr. H. B. Lemon for able assistance in this work, and also to Messrs. Lumière, Cramer, and Seed, who kindly furnished the necessary plates.

ROBERT JAMES WALLACE.

YERKES OBSERVATORY,
July 15, 1905.

A FEW ADDITIONAL FLUOR-SPARS CONTAINING YTTRIUM AND YTTERBIUM

I had hoped sometime greatly to increase my list[1] of fluor-spars containing yttrium and ytterbium, but it is not probable now that I shall soon have the opportunity of doing so. However, the few additional ones that have been kindly sent to me, together with those already reported on, extend the examination practically to all parts of the Earth.

For the sake of this completeness I submit the following supplementary table:

SUPPLEMENTARY LIST OF FLUOR-SPARS EXAMINED

Where from	Furnished by	Amount Yttrium	Amount Ytterbium
AMERICA, *Canada*—			
Cameron, Ontario	R. B.	Large	Appreciable
Lot 4, Concession A, Cobden, Ontario	R. B.	Appreciable	Small
North ½ Lot 13, Concession 3, Cobden, Ontario	R. B.	Small	Small
South ½ Lot 13, Concession 3, Cobden, Ontario	R. B.	Small	Trace
Derry, Quebec	R. B.	Small	
Huell, Quebec	R. B.	Small	
JAPAN—			
Province of Bungo	H. N.		
Province of Ise	H. N.	Trace	
Province of Noto	H. N.	Appreciable	Trace
Province of Tajima	H. N.	Small	

R. B. means Dr. Robert Bell, Geological Survey, Ottawa, Canada. H. N. means Professor H. Nagaoka, Imperial University, Tokyo, Japan.

The fluor-spar from Cameron, Ontario, like the samples from other places, previously reported on, containing large amounts of yttrium, is especially sensitive to thermal effects.

W. J. HUMPHREYS.

UNIVERSITY OF VIRGINIA,
June, 1905.

WAVE-LENGTHS OF CERTAIN SILICON LINES

The three lines at λλ4553, 4568, and 4575 are of especial utility in determinations of the radial velocity of numerous stars having spectra of the *Orion* type. They are particularly prominent and sharp in the subgroup represented by *β Crucis* and *ε Canis Majoris*. The identification

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[1] *Astrophysical Journal*, **20**, 267–270, 1904.

of these stellar lines with silicon was independently and simultaneously accomplished by Sir Norman Lockyer[1] and Mr. Joseph Lunt.[2] They are called "Group III" of the silicon spectrum by the first-named observer.

At the time of publication of the paper on "The Radial Velocities of Twenty Stars" by Frost and Adams, the best available laboratory determination of the wave-lengths of these lines seemed to be those of Exner and Haschek (on the spark spectrum—the lines are not present in the arc spectrum), although the uncertainty of their measures, chiefly due to the diffuse character of the lines, was recognized.[3]

It therefore seemed worth while to attempt a more accurate measurement of the wave-lengths in the spectroscopic laboratory of the Observatory. The plates were all made by Brown[4] with the first order of the 10.5-foot concave grating, using a strong spark between electrodes containing metallic silicon and titanium, probably prepared in an electric furnace. Numerous experiments were tried with a view to overcoming the diffuseness of the lines, but they were hardly successful. The symmetrical character of the lines, however, justified the hope of obtaining more precise measures than those previously published.

The presence of the titanium impurity had the advantage of supplying sharp comparison lines near the silicon lines; but on some of the plates the *Ti* line at λ4552.632 (Rowland's value) evidently was confused with the adjacent silicon line. That line was therefore only measured on plates where the intensity of the *Ti* line was too weak to perceptibly affect the strong silicon line, as could be safely inferred when the *Ti* line at 4555.66, of equal intensity with 4552.63, was too faint to be at all effective.

The current was supplied by a transformer wound 110 to 30,000, with large capacity in parallel with the secondary. The intensity of the lines was increased by enlarging the capacity, while they were made to vanish entirely by putting in self-induction, as has frequently been observed.

Of the plates taken, three were well suited for measurement, and two of these plates were measured by Brown on a Zeiss comparator, with accordant results. The three were later measured by Frost, in each case with violet toward the left and violet toward the right, and with both a single thread and a double thread (actually rulings on glass reticle) on a Gaertner comparator. The silicon wave-lengths are referred to Rowland's values of the titanium lines used, in most cases to λλ 4544.864, 4548.938, 4555.662, 4563.939, 4572.156, and 4590.126 or 4617.452.

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[1] *Proc. R. S.*, **65**, 449, 1899.

[2] *Ibid.*, **66**, 44, 1899.

[3] *Publications of the Yerkes Observatory*, **2**, 157.

[4] Volunteer Research Assistant at Yerkes Observatory, Summer Quarter of 1904.

As Eberhard had found in his valuable "Untersuchungen über das Spektrum des Siliciums"[1] that the sharpness of the silicon lines was increased in an atmosphere (of hydrogen), Professor Crew, in response to our request, kindly offered to have a trial made in his laboratory, with an atmosphere of coal-gas. The desired result was not attained, but an excellent concave-grating plate of the silicon spark in air was made for us by Mr. G. S. Fulcher, to whom, as to Professor Crew, our thanks are due. This plate was measured by Frost, in the same manner as the other plates, twice independently, with an interval of some weeks. The *Ti* line at λ4552 was present on this plate, so that measures were not made of the nearby *Si* line. The two measures of this plate were closely accordant for the two *Si* lines, but in the case of λ4567 the result was quite a little larger than on the plates taken with the Observatory grating.

Inasmuch as the measures by Brown included only two of the plates, and were not made with the violet in both positions under the microscope, it has seemed best to employ only the measures of Frost in deriving the result. The means for the two observers do not differ, however, by 0.01 tenth-meter in case of any of the three lines.

The reductions were made by least squares, by Miss F. A. Graves, in the usual manner for concave-grating plates where several reference lines are used. An idea of the internal agreement of the measures can best be given by printing the results for the different plates. The measures with violet to left and violet to right were combined before they were reduced, so that they cannot be given separately.

WAVE-LENGTHS OF SILICON LINES

Plate	Thread	4553	4568	4575
No. 9..................	Single	4552.65	4567.91	4574.78
	Double	.66	.90	.79
No. 19.................	Single	.64	.87	.80
	Double	.61	.88	.79
No. 24.................	Single	.62	.85	not measured
	Double	.64	.89	
Crew's No. 670......... (Means of two measurements)	Single	not measured	.95	.79
	Double		.94	.80
	Means	**4552.64**	**4567.90**	**4574.79**

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[1] *Zeitschrift für wissenschaftliche Photographie,* **1**, 346, 1903.

The average values of the residuals from the measures of the titanium lines on the plates are as follows: No. 9 (6 *Ti* lines), 0.012 t.-m.; No. 19 (8 *Ti* lines), 0.009 t.-m.; No. 24 (3 *Ti* lines), 0.005 t.-m.; No. 670 (4 *Ti* lines) 0.023 t.-m.

More accurate results can doubtless be obtained when the silicon lines can be made sharper on the spectrograms, and it is planned to experiment with vacuum tubes containing $Si\ Fl_4$, with which Eberhard got the *Si* lines in question sharply defined.

It is proper to add for comparison the results obtained by other observers, viz.:

Gill (from stars)................	4552.79	4567.90	4574.68
McClean (from stars)..........	4552.6	4567.5	4574.5
Lockyer (spark)................	4552.8	4568.0	4574.9
Exner and Haschek (spark)...	4552.75	4567.95	4574.9

The significance of the change to the new wave-lengths from those of Exner and Haschek, previously used in work on *Orion* stars at this Observatory, is best seen when converted into kilometers:

λ4553: Correction (F. and B. − E. and H.) −0.114 tenth-meters = −7.51 km
4568: " " " −0.053 " = −3.48 km
4575: " " " −0.109 " = −7.14 km

The effect on the values of the radial velocities of stars, for which considerable weight was given to the displacements of the silicon lines, is by no means inappreciable. In the case of γ *Pegasi*, for instance, the use of the new values of the *Si* wave-lengths changes the final value of the radial velocity as determined here by fully + 1.5 km.

In due time the precise values of the corrections to be applied to the published radial velocities of other stars of the *Orion* type will be communicated. In many instances these silicon lines were not used.

We incidentally measured the wave-length of the air line (nitrogen) at λ4507, which occurred on most of the plates, with the result: Mean of F. and B., λ = 4507.81. The difference between the results for the two observers was 0.01 t.-m.

EDWIN B. FROST AND JULIUS A. BROWN.

YERKES OBSERVATORY,
August 10, 1905.

Reviews

Guide du Calculateur. Par J. Boccardi. Paris: A. Hermann, 1902. Première Partie, pp. x + 78. Deuxième Partie, pp. viii + 147. Price, Partie I, 4 francs; Partie II, 12 francs.

It is not easy to see why writers of textbooks have afforded so little help to the student who wishes to become proficient in computing; or why M. Boccardi, in the preface to the work that lies before us, should think it necessary to apologize for presenting to us so useful a book. Facility in computing is, to be sure, only to be acquired through practice; but this applies equally well to photography and to many similar arts, none of which can be said to suffer from a lack of works of reference for the beginner. The reviewer has often thought that there is a wide field for missionary work among all classes of professional computers. How many business accountants are there, for example, who know of the existence of such tables as Crelle's? And how few must there be whose labors would not be materially lightened with the aid of that inexpensive work! Ignorance of the tools they might command is even more surprising in the case of engineers, as the reviewer is able to attest from a somewhat extended acquaintance among them.

M. Boccardi divides his work into two parts, published under separate covers. Part I deals with general rules for computing; Part II, with special examples. Part I is again divided into three sections, entitled *before*, *during*, and *after* the calculations. The first of these sections is designed to enable the computer to make an intelligent choice of methods, tables, etc., after having decided upon the degree of accuracy that he wishes to retain. We are glad to see that the author has adequately emphasized the economy of using tables with no more than the necessary number of decimal places—something which is still not as universally recognized as it ought to be. In the reviewer's opinion, the author underestimates the utility of computing machines; his comments upon them are meager and somewhat unfavorable.

Section II, although short, contains so many hints and short-cuts of a practical character that it merits at least the perusal of every computer. Section III points out the best methods for verifying calculations, and for rapid location of errors in them.

Part II is chiefly devoted to perturbations and to the determinations of orbits; it will be found useful by the student who purposes to specialize in this subject, but it has not the general interest that attaches to Part I.

In a work of this character we must expect to find some precepts and details of methods that would not meet with the unanimous approval of computers. For the most part, in M. Boccardi's book, these are matters of opinion at best, and are not of great consequence. Possibly an exception to the last is to be found in the author's dictum (repeated throughout the work in various forms) that "there can never be too many controls." We think it would be better to advise the computer to select one "necessary and sufficient" control, and not to spend too much time on intermediate ones, at least after he has acquired some accuracy in his work.

The size of the page (35×24 centimeters) makes the two volumes somewhat clumsy; otherwise the printer has done his work well. The great merit of the essential features of the book should give it a wide circulation among expert computers as well as beginners.

F. S.

ALLEGHENY OBSERVATORY,
August 1, 1905.

Index to the Literature of the Spectroscope (1887–1900) inclusive. By ALFRED TUCKERMAN. *Smithsonian Miscellaneous Collections*, Vol. XLI, 1902. Pp. 373.

This is a continuation of the previous index by the same author similarly published in 1888, but it represents a decided improvement both in arrangement and accuracy. The rapid development of the subject is indicated by the fact that the present volume, covering fourteen years, nearly equals in size its predecessor, which was intended to include all articles to 1889. This comparison is not entirely fair, however, as the later volume gives more space to each article in the author-index.

The author-index and subject-index divide the book equally. The former gives full titles of articles, and will be found the more generally useful. The latter is subdivided alphabetically into a large number of sections, and this method is followed in the separate sections. This necessarily makes some anomalies: under the section "Astronomical in General" the constellations occur where the alphabet brings them, but without reference to the particular star in the constellation. Thus we have in succession "Cephei," "Cladni" [!], "Comets," "Corona" [only a single reference], "Cygni," "D. M. (Star)," etc. As the subject-index gives only the references to the articles, without their title, it will be of less service than the author-index.

In general, the work will be found helpful, though it has a sufficiency of errors clerical and errors of discrimination. If the author had had a practical acquaintance with more of the articles, the result would have been better, but in that case he would probably have had no time for this bibliographic labor of love; and the workers in the subject certainly should be grateful for this assistance in making available the rather scattered literature.

F.

Popular Star Maps: A Rapid and Easy Method of Finding the Principal Stars. By COMTE DE MIREMONT. Folio, 13×15 inches. London: George Philip & Son, 1904. 10*s.* 6*d.*

These are doubtless the clearest of all star maps for one who wishes to recognize the constellations and locate the principal stars. Ten double maps, 10×10 inches, give on the left-hand page, in white on a blue ground, the stars down to the third or fourth magnitude, with nothing else to distract. On the opposite page are shown the same stars in black on a white ground as usual, with the common name or Greek letter of the star and the constellation names, a few lines joining some of the stars and forming simple figures. The introduction contains the Greek alphabet, a list of the principal constellations, and two indexes, one arranged alphabetically by constellations, the other in order of Right Ascension, giving the name, magnitude, place for 1904, and precession, for each star mapped. A few minor points open to criticism do not materially detract from the excellent adaptation of these maps to the use of the beginner. The declination co-ordinates are not given on the maps and are not needed, hence it is superfluous to carry the declinations in the list to hundredths of a second of arc. The distortion at the corners of the polar maps, for instance in the constellation *Perseus*, masks the resemblance to the sky, and in this case there is no overlapping map to rectify the figure. With the limitations noted, this work admirably fulfils the purpose for which it is intended.

P.

NOTICE

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

If a request is sent *with the manuscript*, one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

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